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LIVING AND LIFELESS MACHINES *

REGINALD O. KAPP

Terms of Reference

WHEN a theme has become the subject of prolonged discussion certain arguments from both sides tend to become rather stereotyped and taken for granted. It then becomes necessary for such arguments to be re-examined, both for their relevance and for the conclusions to which they lead. Even the constructions that have come to be put on the expressions employed in the discussion need, from time to time, to be carefully reviewed. The present paper is an attempt at such a re-examination of the traditional approach to the question : What are the implications of any similarities and differences that there may be between living and lifeless machines ?

The Traditional Arguments

The controversy about this question is philosophically important because of its bearing on the question : Whether living matter is or is not controlled by something that is (*a*) distinct from it, and (*b*) non-material. Those who say that it is so controlled give such names as Life, Mind, Soul, Entelechy, Élan Vital, Diathete to the influence claimed to exercise control. In doing so they attribute the structure and performance of living things to the *aided* action of matter on matter, for they claim that this non-material influence aids the course of material events. By contrast, those who deny that living things are controlled by any sort of non-material influence attribute their structure and performance to the *unaided* action of matter on matter.

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Believers in aided action are sometimes called dualists, while believers in unaided action are called monists. But rather than become involved in a discussion about the justification for these names I shall avoid using them here.

Traditionally, believers in unaided action argue as follows : 'A machine does not contain any substance that one could describe by such a term as Life or Entelechy. The living organism is in all significant features a machine. Therefore there is no reason to postulate any such substance for the living organism.' Recently a number of mechanical devices have been specifically quoted in support of this school. Among them are digital and analogue computers, apparatus so designed that it maintains some quantity at a predetermined value, battery operated trucks encased in a good imitation of the shell of a tortoise, and other ingenious devices. Each is said to support the theory of unaided action in respect of one of the performances of a living organism, the inference being that a sufficient number of such devices, sufficiently varied, would between them cover the whole of life's activities.

Traditionally, believers in aided action reject the view that living organisms are essentially machines. They enumerate sundry accomplishments of which living organisms are and machines are not capable. So it has come to be accepted without question by both sides that to say that a living organism is a machine amounts to saying that it results from the unaided action of matter on matter, and that the proper way of refuting this theory is to prove that it is not a machine.

What is a Machine ?

This seems to me most odd and I hope that it will not be thought paradoxical but merely obvious when I say that protagonists on both sides ought to use exactly the opposite arguments. Believers in aided action ought to insist that living organisms are machines, and believers in unaided action ought to insist that they are not. The error is, I think, that no attempt is made on either side to think out what characterises a machine. For one cannot decide whether a living organism is a machine, nor what conclusions to draw if it is one, unless one has a criterion by which to distinguish between what is and what is not a machine. To say that a living organism is a machine means, if it means anything at all, that it differs in some significant way from a structure that is not a machine. What is this difference ?

It would be correct enough, but quite inadequate to describe a machine merely as a structure in which energy is converted from one form to another. For that holds true of any arbitrary collection of bits and pieces, from a motor car down to a piece of iron that rusts, a rock face that crumbles, or the surface of the sea when the wind disturbs it. It is equally true of any *part* of a motor car, or of an assembly consisting of the wheels, together with a portion of the road, half the house that the car is passing and the boots of a pedestrian. I bring in this absurd illustration in order to make clear, firstly, that the conversion of energy must be in some specific form and, secondly, that there must be a criterion by which to define the boundaries of anything that one would call a machine.

When those who wish to prove the theory of unaided action use an illustration they never choose rusting iron, crumbling rock faces, or disturbed sheets of water ; they never choose a random collection of bits and pieces. They always choose something that performs a wanted operation, something of which one can define the purpose, be it only to maintain a pointer in a predetermined position, something that an engineer too would call a machine, something teleologically determined. If they did not choose something like this the illustration would not be at all convincing.

The Input and Output Functions

A characteristic of all man-made machines is that they serve as instruments of control. They are manipulated at more or less frequent intervals by an operator, who uses handles, levers, pedals, push buttons, thumbscrews, and similar controlling devices ; and their performance is not a random one but the one that the operator contemplates while he is exercising the control. This characteristic has caused engineers to speak of the input function and the output function of some of their machines, of servo-mechanisms for instance. They would be quite precise if they applied the same terms to every machine they worked with. Both an input and an output function can be distinguished for digital and analogue computers, servo-mechanisms, guided missiles set to home on a target, toy tortoises, in short, for every one of the devices quoted in support of the theory of unaided action. And the terms input and output function, be it well understood, do not refer to energy. The input energy and the output energy are distinct from the input and output functions. They have different origins, different destinations, and follow different paths.[1]

To say that the performance of a machine requires an input function is to say that specific things must be done to the machine for it to give its characteristic performance. And every man-made machine is equipped with special devices by means of which an operator can do those things to it. In a motor car they are called the controls. Corresponding devices are an essential constituent of analogue and digital computers, of any machine that an operator so adjusts that it will maintain a pointer in a predetermined position, of a ' robot ' aeroplane, of the mechanical tortoises said to be analogous to living organisms. They are what I have just spoken of as the controlling devices. They serve as receptors for the input function.

Those who say that the structure and performance of a living organism are controlled by a non-material operator distinct from its body say, in effect, that its characteristic output function occurs only when there is or has been a corresponding input function, just as it does for a man-made machine. As with this, they claim, something specific is only done *by* the organism when something specific has been done *to* it. And the living machine, one should expect them to insist, must be equipped with devices analogous to the controls of a motor car, with receptors for an input function, by means of which the assumed non-material operator provides the control or, as I prefer to put it, the diathesis. In other words, one should expect them to insist that a living organism is essentially a machine.

Yet it is their opponents who do this. These opponents, the believers in unaided action, deny that a living organism is controlled by any agent analogous to the operator of a man-made machine ; they deny the need for any mechanisms in a living machine that would correspond to the controls of a motor car. And yet it is they, and not the believers in aided action, who quote in support of their theory the resemblance of living organisms to certain machines.

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[1] As a term that would cover both input function and output function, that would cover indeed every process calling for control, guidance, selection, discrimination, timing I have suggested elsewhere the word ' diathesis ' (see *Science* v *Materialism,* London, 1940, and *Mind, Life and Body,* London, 1951). This term makes it possible to define a machine as a device for converting diathesis from one form into another. But I do not need to use the term here. The more familiar terms ' input function ' and ' output function ' serve the present purpose equally well.

This is why, as I remarked earlier, the position of the contesting parties is inverted. Those who ought to argue that living organisms are machines do not and their opponents do.

Anyone who believes in *aided* action ought, if he thought the matter out, to argue thus : 'I support the hypothesis that a living organism does not exhibit its characteristic output function when subjected only to random forces. I claim that it must be controlled by some influence that is distinct from itself. I claim that, in technical terms, there is an input function whenever the organism exhibits its characteristic behaviour. The same is true of a machine. So the lifelike performance of this mechanical tortoise tends to support my hypothesis in so far as similar output functions justify the assumption of the reality of an input function for both.'

Anyone who believes in *unaided* action ought, on the other hand, to argue thus : ' I support the hypothesis that a living organism does exhibit its characteristic output function when left entirely to the random influences of its environment. I do not believe that it is controlled at all in the sense in which a man-made machine is controlled. I do not accept the view that there is anything that an engineer would recognise as an input function. In this respect every living organism differs basically from any device that human ingenuity can produce. This mechanical tortoise, for instance, does not work if left entirely to the random forces of its environment. It only exhibits its characteristic output function when I manipulate it in certain ways, when I operate certain controls and make certain adjustments. It would be quite wrong to jump to the conclusion that the lifelike performance of this ingenious device proves the need for an input function for a live tortoise. The distinction between man-made and living machines is basic ; it is that there is an input function for the former and none for the latter.'

This, I repeat, is how both sides ought to argue. But instead believers in aided action deprecate the machine analogy. And believers in unaided action insist on it. They attempt to prove that the living organism does not need an operator distinct from itself by pointing to machines that do. From the resemblance of a living organism to a machine that has an input function they infer that the living organism has none. Surely it can only be in the Colleges of Unreason, described by Samuel Butler, that the traditional arguments can have originated. It is only there that one might expect so much attention to be given to the output function, which is barely relevant to the controversy, while the input function, which is very relevant, is being assiduously ignored.

The Hypothesis that Living Organisms are Automatic Machines

A bad advocate may yet have a good case. So let us consider next what facts and reasoning would support belief in unaided action.

To justify such belief one would clearly have to prove that there was no input function to a living organism. And to say that there is no input function is to say that the living organism is 100 per cent automatic in the sense of being a self-constructing, self-operating, self-repairing, self-maintaining machine. This is what believers in unaided action must and do say even when they try to prove it by means of experiments and demonstrations with machines that are definitely not self-constructing, not self-operating, not self-repairing, not self-maintaining.

Some man-made machines are more automatic than others. And the degree to which a machine is automatic is a measure of the amount of control and adjustment that must be applied to it. In a very automatic machine the number of separate controlling devices is small and these only need to be operated at rare intervals. The driver of a very automatic engine can leave it alone for long intervals after he has started it up, and many parts of the engine may work satisfactorily for years before they require any attention whatever. The driver of a motor car equipped with automatic gear change need do less things with his hands and feet than if the gear is not automatic. A robot could be left without attention for long periods. The input functions might be reduced to the occasional adjustment of a few thumbscrews and some words spoken into a microphone. A self-winding watch keeps good time if adjusted only every few years.

A machine that was 100 per cent automatic would, by definition, not even require that much input function. Whether such a machine could be produced in a factory is not relevant. One relevant question is whether it is possible in theory, whether the concept of a machine that is 100 per cent automatic is consistent with physical science. This is an interesting question but I do not propose to discuss it now.

The question that I do propose to discuss is the more concrete one, whether observation of living organisms tends to support or to refute the hypothesis that they are 100 per cent automatic. For this is, be it remembered, a hypothesis and as such in need of support. Moreover, it is fundamental in the dispute and has not been discussed in the literature. The present position is that both disputants can at present have recourse only to faith : on the one side to faith in the existence of non-material influences and on the other side to faith in the powers and accomplishments of matter. If this is not always appreciated it is because of a human weakness for describing one's own faith as facts and one's opponent's as mere hypothesis.

On the question, automatic or not automatic, I find again that the traditional view, blindly accepted by both sides, is the very opposite of the view to which facts of observation point. It always seems to be taken for granted that only those performances of living organisms that are subject to conscious control can conceivably be non-automatic. All other, and particularly purely vegetative processes, are usually declared to be 100 per cent automatic both by believers in aided and in unaided action. And yet no facts of observation support the hypothesis that living organisms are 100 per cent automatic ; on the contrary, evidence available to everyone, as I shall show in a moment, strongly supports the hypothesis that living organisms are nearly, though not quite, 100 per cent non-automatic.[1] Before we come to this evidence let us consider the very minimum physical constituents of any machine, be it living or lifeless.

The Essential Constituents of a Machine

There are, and always must be, three indispensable constituents, namely : moving parts, fixed parts, and a source of energy.

That some things move in any machine is obvious. In moving they transmit forces, they exert pushes and pulls. It is equally obvious that there must be something to push and pull against. This is the frame. So any machine, living or lifeless must contain some parts that are fixed relative to others that move.

Among the moving parts in a motor car engine are pistons, connecting rods, shafts, wheels, valves, levers. These are guided and restrained by portions of the frame. This latter includes the bedplate, holding-down bolts, bearings, cylinders, cylinder covers, casings, struts, tie-rods, containers for fuel and lubricants. The frame of a motor car engine must be adapted to its function, which means that it must be strong, rigid and durable. So must each individual moving part. It is designed so that it may not bend or buckle, break or tear under the imposed stresses. Surfaces subject to wear must be hard and smooth.

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[1] As the hypothesis has recently been supported by some distinguished scientists that properly designed machines could both think and reproduce their kind it may be advisable for me to make it clear that I propose, on this occasion, to discuss only observable performances. And I do not propose to discuss either capacity for thought or capacity for reproduction.

The properties required of the three main constituents are often mutually exclusive. Joints between moving parts must be loose ; they are often hinged. Joints between parts of the frame must be rigid ; they are bolted, riveted or welded. Moving parts must be light and most of them have a small inertia ; a well-designed frame is comparatively heavy. So far as possible the moving parts and frame are made of substances that do not burn easily ; but this is exactly what the fuel must do. It lacks the property of durability essential for the moving parts and the frame, and has the property of inflammability lacking in them.

The Basic Difference between Living and Lifeless Machines

A living engine must, as I have said already, also consist of moving parts, frame and fuel. And I want to point out how basically the constituent parts of a living machine differ from the corresponding constituents of any man-made one. One might think at this stage that such a comparison is trivial and of no great philosophical significance, and that a true philosopher would ignore it in favour of other and bigger distinctions, such as capacity for thought, capacity for reproduction, a sense of values, higher things. But perhaps the notion will obtrude, as I proceed, that the real reason for reluctance to give attention to these problems of mechanics may be that they introduce some awkward questions.

In a man-made machine it is easy enough to distinguish on a casual inspection between the three essential constituents. One would not mistake the petrol for the bedplate nor a rivet for a cam. One should expect, therefore, when viewing a living machine to be able to tell quickly which parts were moving ones, which the frame and which the fuel. The properties of the materials and the shape of the items ought to provide an easy clue. In a man-made machine the materials used and their shapes have to be distinctive enough if the engine is to have a good thermal efficiency, to be light, to work without backlash, to last for a sufficient time without repair.

Let us list then first the moving parts of the human body. They include, of course, the legs, the arms, the hands, the fingers. The head is another moving part ; it can nod and turn. The upper and lower portions of the trunk can each move relative to the other. In fact one cannot name one major division of the human body that is not a moving part.

A glance at the skeleton shows a design well adapted to this. Joints are hinged and not riveted, bolted or welded, as they would be if mobility were not essential. Similarly soft tissues are designed to yield. When one examines them one finds that they are designed for mobility. And when one examines the constituent parts of any soft organ one finds that these, too, are moving parts. The tongue, the heart, the walls of blood vessels, individual muscle fibres, nearly all components, right down below microscopic dimensions, are well adapted to the function of mobility.

Where then is the frame ? Is the living engine a curious kind of machine built of nothing but moving parts ? Certainly not. If it were it could not work. It would collapse. By the principles of mechanics there must be a frame. Where is it ?

There is, of course, no mystery. At any moment certain parts of the body cease to be moving parts and, for that moment, develop the function of the frame. They are altered to serve the function. They then take up the pushes and pulls exerted by the parts that are, at that moment, moving ones. Whenever this happens the shape of each component is temporarily changed. In other words, a significant difference between a living and a lifeless machine is that for the latter the frame is permanent, while for the former it is temporary. It is an *ad hoc* frame. While one takes one step the stresses within the body are constantly changing and those parts used at the moment to take up these stresses are changed in shape accordingly. Components that are tie-rods at one moment become struts at another and connecting rods at yet another. While one word is being pronounced the tongue and lips change their shape and function many times. It is the same with all internal parts down to each tiny cell. Every component of a living organism is a machine that works in a strange way indeed. There is continuous redesigning of all its minutest component parts.

The Principle of Continuous Redesigning

This redesigning explains the high weight efficiency of living substance. The theme is a big one and I regret that there is not time now to discuss it adequately. Let me be content merely to point out that a man-made walking machine with a permanent frame would have to be so designed that this frame would take up stresses when the heel was touching the ground as well as those others that occurred when the toe was doing so. But the living machine is such that while the heel is touching the ground there is nothing capable of taking up those stresses that will be imposed at the next moment as the toe comes down. The frame will be correctly redesigned at the right moment for this and will then no longer have anything that would resist the stresses imposed by the posture just abandoned. Similarly a motor car must be permanently fitted with a brake as well as an engine. But an animal has no brake while it is running. It is redesigned so as to acquire one when it is coming to a stop ; and at this moment it ceases to have an engine of propulsion, an engine designed for the function of running. Any boxing machine would have to be equipped both with thrusting and parrying devices. A human boxer has each when needed and not at other times. An arm is a device for delivering a blow at one moment and a shield at the next.

Thus the conversion of the living body from one kind of machine to another is as radical as the conversion of swords into ploughshares. And it goes on so long as the organism is alive. In engineering such redesigning would call for a new set of blueprints, a new set of calculations many times a second. Can anyone seriously think that digital and analogue computers, toy tortoises, guided missiles come anywhere near to being a true analogy ? For a true analogy it would not suffice that the toy tortoises were made of soft materials that the operator could easily bend and twist into new shapes. Devices for doing the twisting and bending would have to be incorporated in those soft materials. In other words many subsidiary machines would have to be provided. Their function would be to control the shape of the tissues from moment to moment. And the subsidiary machines would, in turn, have to be made up of sub-subsidiary machines and so forth down to molecular dimensions. The operator of a toy tortoise that was even remotely like a live one would be kept busy to bring about all the changes in the shape of the component parts that occur in the live one.

I have noticed that a fact of common observation is often not properly appreciated until it is given a name. So let me find one for this weight-saving method by which living organisms do not embody any component parts that are not required for the function of the moment. I shall call it the Principle of Continuous Redesigning. It is one of the most basic of biological principles and deserves, indeed, much more discussion than there is time for on this occasion.

To the illustrations of this principle already given, further ones could be added without limit drawn both from large scale and micro-physiology. But even if time permitted, additional illustrations would do little to make the Principle of Continuous Redesigning more self-evident. After all, anyone can easily find an unlimited number of further illustrations for himself.

The Principle of Continuous Repair and Maintenance

Let us now turn our attention to the fuel. In a motor car this is petrol and it is stored in a tank. In the human body it is chiefly glycogen and is stored in the muscles, having been converted from glucose in the liver. The chemical processes during muscle activity include the combination of muscle protein with sodium. This protein is therefore another part of the fuel. So both the glycogen and the protein serve the double function of being fuel and being constituents of those muscle fibres that are at one moment moving parts and at another components of the frame. The living body is analogous to a motor car in which the chassis, brakes, cylinders, pistons, connecting rods, valves and bearings all contained combustible material, some of which was burnt whenever the driver placed his foot on the accelerator. By engineering principles such an engine could not work. Depleted of their substance supports would weaken and collapse, tie-rods would break, struts buckle ; valves and pistons would cease to be a tight fit ; every working part would develop backlash. A motor car built to the principles adopted by life would fall to pieces before it had covered a mile. Indeed an engineer could only say of the living body : ‘ This is not built to last. This is built to wear out.’

Any man-made machine in which surfaces of the frame and moving parts are subject to much wear requires correspondingly frequent attention. Wear is corrected by replacing what has been worn away. So it is with the living body. If matter is worn away at a rate vastly exceeding that in any man-made machine, it is replaced at a corresponding rate. To the Principle of Continuous Redesigning one must add the Principle of Continuous Repair and Maintenance.

A Permeating Input Function

What is true of control in time is equally true of control in space. Large portions of the volume of any man-made machine never need to have anything done to them. This applies, for instance, to the inside of a shaft or a bedplate. But for a living machine one cannot say that the inside of any morsel of tissue is not subjected to controlled change. Even bones are the sites of much and varied activity. The tiniest membrane in a cell must conform to this rule. There is, in my terminology, a micro-diathesis in all living substance. In more colloquial terms, the living machine is analogous to a motor car for which the controls are not limited to a few items, such as the steering wheel, pedals, clutch handle and so forth, but occur throughout the structure. If one could make a toy tortoise that truly resembled a live one, the input function would not only have to be continuous ; the controlling devices that enabled the operator to supply the input function would have to be legion. And they would have to be distributed throughout the tissues. The things done *to* such a device would have to be permeating as well as unremitting. An operator who had to cope with such a machine would surely not say that it was 100 per cent automatic ! One would have to go to the Colleges of Unreason to meet that interpretation.

Summary and Conclusion

To summarise. The available facts point to a more basic resemblance between living and lifeless machines than has generally been recognised on either side of the dispute. Both kinds of machine are distinguishable from any structure that is not a machine in two basic features. Firstly, as frequently stressed by others, both exhibit characteristic output functions. Secondly, though less often pointed out in the past, the facts suggest that input functions are necessary for both. The most fundamental difference between the two types of machine is in the nature of the input function. For a man-made machine it is applied only occasionally and at a few places. For a living machine one must assume, at least until opposing facts have been found, that the input function is (*a*) continuous (*b*) permeating, and (*c*) operating on a scale too small for any material manipulator, indeed on a scale at which a Clerk Maxwell demon would work.

Thus certain hard facts tell against the hypothesis that living organisms are 100 per cent automatic. And these facts suggest that the control to which they are subjected is not applied only to the organism's *conscious* activities, but to *all* of them including even vegetative ones. The activity of what I prefer to call a diathete is of a nature that one cannot grasp with one's imagination. Is there any need that one should ?

It may be argued that these hard facts will one day be interpreted in a way consistent with the hypothesis of unaided action. Perhaps. But if it is to be so the hard facts must be faced and an interpretation that will save the hypothesis deliberately sought. Until it has been found the hypothesis of unaided action cannot be maintained with the support of, but in spite of, observable facts. Its basis must continue to be pure faith.

Whether this conclusion will appear attractive or unattractive I do not know. But I think one major obstacle to its ready acceptance may be difficulty in finding a satisfactory answer to the question : Where is the operator that is assumed to be in control of this continuous and permeating input function ? It is therefore worthwhile to point out that this question cannot have a meaning. For if the facts oblige one to assume the activity of a non-material influence in control of any living substance, the facts also force one, for reasons that it would take too long to explain here, to assume that such an influence must be without location.

LOGIC AND EXISTENCE*

Czesław Lejewski

I have given my essay this title because it roughly indicates the boundaries of the topic to be discussed and at the same time hints at the method that will be adopted in my analysis. The problem of existence will interest me only to the extent to which it enters the province of logical enquiry and I shall try to disentangle it a little by departing from the generally accepted interpretation of the quantifiers and by bringing in other concepts related to that of existence.

When we have to commit ourselves to asserting or rejecting propositions like

electrons exist (1)
minds exist (2)
Pegasus exists, (3)

our hesitation can be traced to twofold causes. In the first place we may not be willing to give our judgment because we are not quite certain what we mean by 'electrons' or 'minds', or we may not understand the word 'Pegasus'. In the second place we may be confused as regards the meaning of the term 'exist(s)'. It is the latter cause of our embarrassment that calls for closer attention. Let the physicist, the psychologist, and the mythologist deal with the meaning of the words 'electrons', 'minds', and 'Pegasus' respectively. The logician's task, as I understand it, will be to establish the meaning of the constant term 'exist(s)' as it occurs in the function '*x* exist(s)' where '*x*' is a variable for which any noun-expression can be substituted.

I hope that it will be a permissible simplification to say that in recent times the discussion over the logical side of the problem of existence centres around what Professor Quine has written on the subject.[1] In presenting the views of this author I shall have to use a little more quotation than is customary as the whole matter is of exceptional subtlety.

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* The first draft of this paper was presented to a post-graduate seminar at the London School of Economics on 12th November 1953, and was also read and criticised by Professors J. Łukasiewicz, K. R. Popper, W. V. Quine, and J. H. Woodger, from whose generous comments I have benefited much.

[1] Quine's most important contributions in this connection are the following: 'A Logical Approach to the Ontological Problem', *Journal of Unified Science*, Chicago, 1940, **9**. This paper was read at the Fifth International Congress for the

On page 150 of Quine's *Mathematical Logic* we read :

> To say that *something* does not *exist*, or that there *is* something which *is not*, is clearly a contradiction in terms ; hence ' $(x)(x \text{ exists})$ ' must be true.

Let us translate this argument into a symbolic language so that its logical structure may become more perceptible. I think that the following translation stands a fair chance of being acceptable to Quine :

$$(\exists x)\,(x \text{ does not exist}) \equiv (\exists x)\,(x \text{ exists} \,.\, \sim (x \text{ exists})) \qquad (4)$$

If instead of ' x does not exist ' in the antecedent of (4) we write ' $\sim (x \text{ exists})$ ' then from (4) and from the circumstance that the consequent of (4), being a contradiction, is false, we get immediately

$$\sim (\exists x) \sim (x \text{ exists}), \qquad (5)$$

which in accordance with the law relating the existential quantifier to its universal counterpart is equivalent to

$$(x)(x \text{ exists}). \qquad (6)$$

This result seems to confirm our interpretation of the passage, which is in complete harmony with the opening paragraph of ' On What There Is ' as this paragraph runs as follows :

> A curious thing about the ontological problem is its simplicity. It can be put into three Anglo-Saxon monosyllables : ' What is there ? ' It can be answered moreover in a word — ' Everything '—and everyone will accept this answer as true.[1]

But let us revert to page 150 of *Mathematical Logic*. The passage which we began to analyse just now continues thus :

> Moreover we should certainly expect leave to put any primitive name of our language for the ' x ' of any matrix ' . . . x . . . ', and to infer

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Unity of Science, Cambridge (Mass.), 1939 ; ' Designation and Existence ', *The Journal of Philosophy*, New York, 1939, **36**, also in *Readings in Philosophical Analysis*, edited by H. Feigl and W. Sellars, New York, 1949 ; ' Notes on Existence and Necessity ', *The Journal of Philosophy*, New York, 1943, **40** ; ' On What There Is ', *The Review of Metaphysics*, New Haven, 1948, **2**, also reprinted in *Proceedings of the Aristotelian Society, Supplementary Volume XXV*, London, 1951.

This list would have to be supplemented with the titles of several, more technical papers, published by Quine in *The Journal of Symbolic Logic*, and also with some passages from his *Mathematical Logic*, Cambridge (Mass.), 1947 (second printing), and *Methods of Logic*, London, 1952.

[1] See W. V. Quine, ' On What There Is ', *The Review of Metaphysics*, New Haven, 1948, **2**, 21

> the resulting singular statement from ' $(x)(\ldots x \ldots)$ ' ; it is difficult to contemplate any alternative logical rule for reasoning with names.

This logical rule, which in *Methods of Logic* is referred to by Quine as the rule of universal instantiation, owes its validity to a certain logical law which with the aid of symbols can be expressed in the following way :

$$(x)\,(Fx) \supset Fy. \tag{7}$$

Now difficulties begin to appear and Quine sets them out as follows:

> But this rule of inference leads from the truth ' (x)(x exists) ' not only to the true conclusion ' Europe exists ' but also to the controversial conclusion ' God exists ' and the false conclusion ' Pegasus exists ' if we accept ' Europe ', ' God ', and ' Pegasus ' as primitive names in our language. [1]

From the whole passage quoted from page 150 of *Mathematical Logic* we seem to be entitled to conclude that for Quine (6), i.e. ' (x)(x exists) ', is a truth while (3), i.e. ' Pegasus exists ', is a falsehood. Regarding the rule which allows us to infer ' Fy ' from ' $(x)(Fx)$ ', Quine cautiously remarks that

> it is difficult to contemplate any alternative logical rule for reasoning with names.

He is more outspoken in his ' Notes on Existence and Necessity ', to which we now turn.

In that paper Quine discusses inferences which could be exemplified by the one whereby from

$$\text{Pegasus does not exist} \tag{8}$$

we infer

$$(\exists x)\,(x \text{ does not exist}) \tag{9}$$

in virtue of the rule which allows us to infer ' $(\exists x)(Fx)$ ' from ' Fy '.[2] In ' Notes on Existence and Necessity ' and in *Methods of Logic* this rule is described as the rule of existential generalisation, and we may add at once that it derives its validity from the following logical law :

$$Fy \supset (\exists x)\,(Fx). \tag{10}$$

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[1] See W. V. Quine, *Mathematical Logic*, Cambridge (Mass.), 1947, 150

[2] See W. V. Quine, ' Notes on Existence and Necessity ', *The Journal of Philosophy*, New York, 1943, **40**. The inference has been rephrased so that it may conform with the example taken from *Mathematical Logic*. The original propositions are ' There is no such thing as Pegasus ' and ' $\exists x$ (there is no such thing as x) ', respectively. See op. cit. 116.

According to Quine (8) would be true but (9) would be false. Regarding the rule he observes the following :

> The idea behind such inference is that whatever is true of the object designated by a given substantive is true of something ; and clearly the inference loses its justification when the substantive in question does not happen to designate.[1]

I think that we have come to a point where a brief summary of Quine's argument may not appear to be superfluous. We have two inferences :

Inference I

From

$$(x)(x \text{ exists})$$

we infer

$$\text{Pegasus exists}$$

by universal instantiation.

Inference II

From

$$\text{Pegasus does not exist}$$

we infer

$$(\exists x)(x \text{ does not exist})$$

by existential generalisation.

According to Quine both these inferences are objectionable to our intuitions. In his opinion they lead from truths, ' $(x)(x$ exists) ' and ' Pegasus does not exist ', to falsehoods, ' Pegasus exists ' and ' $(\exists x)(x$ does not exist) '. In other words the logical laws (7) and (10), which we thought to be behind the rule of universal instantiation and the rule of existential generalisation, do not hold for every interpretation of ' F ' and every substitution for the free variable. If we interpret ' F ' as ' exists ' and substitute ' Pegasus ' for ' y ', (7) turns out to be false. Similarly, (10) turns out to be false for the same substitution if we interpret ' F ' as ' does not exist '. But no difficulty arises if the noun-expressions substituted for ' y ' in (7) and (10) are non-empty :

$$(x)(x \text{ exists}) \supset \text{Socrates exists} \tag{11}$$

$$\text{Socrates does not exist} \supset (\exists x)(x \text{ does not exist}) \tag{12}$$

are true propositions.

A remedy that might suggest itself to an unscrupulous mind would be to ban the use of empty noun-expressions and consider them as meaningless. Quine is right in not following this course. One may disagree as to the truth-value of the proposition 'Pegasus exists' but one would have to have attained an exceptionally high degree of sophistication to contend that the expression was meaningless. Quine does not think that empty noun-expressions are meaningless just because they do not designate anything. He allows for the use of such words as 'Pegasus', 'Cerberus', 'centaur', etc. under certain restrictions and tries to distinguish between logical laws which prove to be true for any noun-expressions, empty or non-empty, and those which hold for non-empty noun-expressions only. It follows from his remarks that before we can safely use certain laws established by logic, we have to find out whether the noun-expressions we may like to employ, are empty or not. This, however, seems to be a purely empirical question. Furthermore, all the restrictions which according to Quine must be observed whenever we reason with empty noun-expressions, will have to be observed also in the case of noun-expressions of which we do not know whether they are empty or not.

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[1] See W. V. Quine, op. cit. 116

This state of affairs does not seem to be very satisfactory. The idea that some of our rules of inference should depend on empirical information, which may or may not be forthcoming, is so foreign to the character of logical enquiry that a thorough re-examination of the two inferences may prove to be worth our while. Let us then try to find out on what grounds (6) is asserted as true while (9) is rejected as false, and let us find out also on what grounds the rules of universal instantiation and existential generalisation are regarded as inapplicable to reasoning with empty names. In seeking answers to the above questions we shall have to turn to the interpretation of the quantifiers.

Regarding Quine's interpretation of the quantifiers, which is the one accepted by the majority of modern logicians, we have a very useful passage in the *Methods of Logic*. It reads as follows:

> If we think of the universe as limited to a finite set of objects a, b, $\ldots$, h, we can expand existential quantifications into alternations and universal quantifications into conjunctions; '$(\exists x)Fx$' and '$(x)Fx$' become respectively:
>
> $$Fa \vee Fb \vee \ldots \vee Fh, \qquad Fa \,.\, Fb \,.\,.\,.\,.\,.\, Fh$$[1]

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[1] See W. V. Quine, *Methods of Logic*, London, 1952, 88

To have a still simpler though fictitious example let us think of the universe as limited to two objects **a** and **b**. Then the corresponding expansions would be :

$$F\mathbf{a} \vee F\mathbf{b} \quad \text{and} \quad F\mathbf{a} \,.\, F\mathbf{b}$$

Our language, which for reasons of simplicity needs not synonyms, may leave room for noun-expressions other than the singular names '**a**' and '**b**'. We may wish to have a noun-expression '**c**' which would designate neither of the two objects, in other words which would be empty, and also a noun-expression '**d**' which would designate either. Introducing noun-expressions is a linguistic matter. It does not affect our assumed universe, which continues to consist of **a** and **b** only. The new noun-expressions can now be put to use. For instance we can form the following true proposition ; '**c** does not exist', and on turning '**d**' into a predicate-expression '**D**' we can further assert that '$(x)(\mathbf{D}x)$' is true. To say that something exists means the same as to say that it belongs to the universe. Thus '**a** exists' and '**b** exists' are true propositions. From the expansion of the existential quantification we see that either of these two propositions implies '$(\exists x)$(x exists)'. But we have no ground to contend that '**c** does not exist' implies '$(\exists x)$(x does not exist)'. Again since '**a** does not exist or **b** does not exist' is false, we conclude from the expansion that '$(\exists x)$(x does not exist)' is also false. Hence '(x)(x exists)' is true. This is confirmed by the expansion of the universal quantification and in view of the circumstance that '**a** exists and **b** exists' is true. We also learn from this expansion that '(x)(x exists)' does not imply '**c** exists'. Within our fictitious universe the rule of universal instantiation and the rule of existential generalisation fail if applied to '**c**' or '**d**'. They are valid rules of inference if their application is restricted to reasoning with '**a**' and '**b**'.

But this is not the only possible interpretation of the quantifiers. With the aid of the same fictitious example I shall now present an interpretation which as far as I can judge, is in harmony with the one adopted for instance by Leśniewski of the Warsaw School.

Our universe consisting of **a** and **b** remains the same but the quantifiers read differently. Under the present interpretation '$(\exists x)(Fx)$' becomes :

$$F\mathbf{a} \vee F\mathbf{b} \vee F\mathbf{c} \vee F\mathbf{d}$$

Correspondingly, '$(x)(Fx)$' is to mean :

$$F\mathbf{a} \,.\, F\mathbf{b} \,.\, F\mathbf{c} \,.\, F\mathbf{d}$$

Thus one can argue that ' $(\exists x)(x$ does not exist$)$ ' is true because ' **c** does not exist ', which under the new interpretation is one of the components of the existential expansion, is true. Hence ' $(x)(x$ exists$)$ ' is false. This is confirmed by the corresponding universal expansion, which contains a false component, namely ' **c** exists '. Under this interpretation the rule of universal instantiation and the rule of existential generalisation are valid without any restrictions. They can be safely applied in reasoning with any noun-expressions : singular non-empty like ' **a** ' and ' **b** ', empty like ' **c** ', or general non-empty like ' **d** '. The noun-expression ' **d** ' need not be changed into the corresponding predicate-expression ' **D** '.

These two different interpretations of the quantifiers, which in what follows, will be referred to as the *restricted* interpretation and the *unrestricted* interpretation respectively, can now be generalised to apply to a universe with any number of objects. In the case of a finite universe we have finite expansions which are equivalent to their respective quantifications. If, however, we think of the universe as consisting of an infinite or unknown number of objects then we cannot have equivalences for the simple reason that we can never form complete expansions of our quantifications. Consequently we abandon equivalences in favour of implications. We say that an existential quantification is implied by any component of its infinite or unknown expansion and that a universal quantification implies any component of its infinite or unknown expansion. Now the expansions will vary depending on how we choose to interpret the quantifiers. Under the restricted interpretation every component of an expansion contains a noun-expression which designates only one of the objects belonging to the universe. Under the unrestricted interpretation every component of an expansion contains an expression of which we can only say that it is a meaningful noun-expression. It may designate only one of the objects belonging to the universe, it may designate more than one, or it may designate nothing at all.

The two interpretations of the quantifiers give rise to two different theories of quantification and we may well be expected to say a few words on the relation in which one theory stands to the other. In this respect the most important point is that whatever is said with the aid of the theory of restricted quantification, can be easily expressed in terms of the unrestricted quantification provided we are allowed to use the notion of existence. A few examples set out below in the form of two lists will suffice to illustrate the procedure :

I. Expressions to be understood in the light of the restricted interpretation :

$$(x)(Fx) \supset Fy \qquad (7)$$

$$Fy \supset (\exists x)(Fx) \qquad (10)$$

$$(x)(Fx) \equiv \sim (\exists x)(\sim Fx) \qquad (13)$$

II. Corresponding translations to be understood in the light of the unrestricted interpretation :

$$(x)(x \text{ exists} \supset Fx) \supset Fy \qquad (7a)$$

$$Fy \supset (\exists x)(x \text{ exists} \,.\, Fx) \qquad (10a)$$

$$(x)(x \text{ exists} \supset Fx) \equiv \sim (\exists x)(x \text{ exists} \,.\, \sim Fx) \qquad (13a)$$

It is not difficult to check that under their respective interpretations the corresponding expressions in the two lists yield the same truth value for the same substitutions performed on the free variables regardless of how we choose to interpret the predicate letters. The general rule for translating expressions is simple : expressions of type ' $(x)(Fx)$ ' and ' $(\exists x)(Fx)$ ' become expressions of type ' $(x)(x$ exists $\supset Fx)$ ' and ' $(\exists x)(x$ exists . $Fx)$ ' respectively ; other expressions remain unchanged.

Thus, for instance, (6) and (9) translated for the purpose of the unrestricted interpretation become

$$(x)(x \text{ exists} \supset x \text{ exists}) \qquad (14)$$

and

$$(\exists x)(x \text{ exists} \,.\, x \text{ does not exist}) \qquad (15)$$

respectively. These translations fully account for the assertion of (6) and the rejection of (9) under the restricted interpretation of the quantifiers. Similarly (7*a*) and (10*a*) make it clear why (7) and (10) do not turn out true for all interpretations of ' F ' and for all substitutions for the free variable. For if we interpret ' F ' as ' exists ' and substitute say ' Pegasus ' for ' y ', the antecedent of (7*a*) becomes a tautology but at the same time the consequent turns out to be false. And again, if we interpret ' F ' as ' does not exist ' and substitute ' Pegasus ' for ' y ' then the antecedent of (10*a*) comes out true but the consequent must be rejected as a contradiction. Thus for certain interpretations of ' F ' (7) and (10) turn out to be false if we substitute an empty noun-expression for the free variable. Consequently the rules of universal instantiation and existential generalisation, which derive their validity from (7) and (10), can no longer be applied without restrictions.

The position is different if we choose to understand the quantifiers in the light of the unrestricted interpretation. (3), which is 'Pegasus exists', and (8), which is 'Pegasus does not exist', are meaningful propositions either of which contains a noun-expression, viz. 'Pegasus'. Thus (3) and (8) may be regarded as components of quantificational expansions. Now, (3) being false, the corresponding universal quantification, i.e. '$(x)(x$ exists$)$', which ought to imply any component of its expansion, must also be false. On the other hand, (8) being a true proposition the corresponding existential quantification, i.e. '$(\exists x)(x$ does not exist$)$', which is implied by any component of its expansion, must also be true. Thus under the unrestricted interpretation of the quantifiers the two inferences on p. 107 cannot be used as counter examples to disprove the validity of the rules of universal instantiation and existential generalisation in application to reasoning with empty noun-expressions. In Inference I both the premiss and the conclusion are false, while in Inference II both the premiss and the conclusion are true. There is nothing wrong with the inferences provided we adopt the unrestricted interpretation of the quantifiers. Furthermore, under the unrestricted interpretation the logical laws (7) and (10) turn out to be universally true. For every proposition of type 'Fa' where 'a' stands for any noun-expression, empty or non-empty, is now regarded as a component of the quantificational expansions and consequently is implied by the corresponding proposition of type '$(x)(Fx)$' and implies, in turn, the corresponding proposition of type '$(\exists x)(Fx)$'. (7) and (10) being universally true, the rule of universal instantiation and the rule of existential generalisation are universally valid as the principles that are behind them are no longer principles by courtesy.[1]

The unrestricted interpretation of the quantifiers seems to remove yet another difficulty from quantification theory. It has been argued by several authors that

$$(\exists x)(Fx \vee \sim Fx) \tag{16}$$

and

$$(x)(Fx) \supset (\exists x)(Fx), \tag{17}$$

which are valid if the universe is not empty, fail for the empty universe as their truth depends on their being something.[2] When discussing these laws Quine tries to dismiss the case of the empty universe as relatively pointless and reminds us that in arguments worthy of quantification theory the universe is known or confidently believed to be non-empty.[1] This contention, however, does not quite remove our uneasiness particularly as (16) and (17), not unlike (7) and (10), are demonstrable in quantification theory.

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[1] See W. V. Quine, 'Notes on Existence and Necessity', *The Journal of Philosophy*, New Haven, 1943, **40**, 118

[2] For details see W. V. Quine, *From a Logical Point of View*, Cambridge (Mass.), 1953, 160 sq.

On considering (16) and (17) we readily admit that these two formulae fail for the empty universe if we understand the quantifiers in accordance with the restricted interpretation. This becomes evident from

$$(\exists x)(x \text{ exists} \,.\, (Fx \vee \sim Fx)) \tag{16a}$$

and

$$(x)(x \text{ exists} \supset Fx) \supset (\exists x)(x \text{ exists} \,.\, Fx), \tag{17a}$$

which are the corresponding translations of (16) and (17) to be understood in the light of the unrestricted interpretation. If there exists nothing then (16*a*) and the consequent of (17*a*) are obviously false while the antecedent of (17*a*) is obviously true. Under the unrestricted interpretation, however, (16) and (17) come out to be true irrespective of whether the universe is empty or non-empty. For (16) is implied by any component of type '$Fa \vee \sim Fa$' where 'a' stands for a noun-expression. In particular it is implied by a component '$Fa \vee \sim Fa$' in which 'a' stands for an empty noun-expression. Such a component is true for all choices of universe and so is (16). In the case of (17) we argue as follows: if we assume that the antecedent of (17) is true then a proposition of type 'Fa' where 'a' stands for an empty noun-expression must also be true in harmony with the unrestricted interpretation of the universal quantifier. Now any such proposition implies the proposition of type '$(\exists x)(Fx)$', which again must be true. Thus in the establishing of the truth value of (16) and (17) the problem of whether the universe is empty or non-empty is altogether irrelevant on condition, of course, that we adopt the unrestricted interpretation of the quantifiers.

It ought to be evident from what has already been said that under the unrestricted interpretation existential quantifications have no existential import. In fact it would be misleading to read '$(\exists x)(Fx)$' as 'there exists an x such that Fx'. The non-committal 'for some x, Fx' seems to be more appropriate. Similarly the terms 'existential quantification' and 'existential quantifier' no longer apply and could be conveniently replaced by such expressions as 'particular quantification' and 'particular quantifier'. The rule of existential generalisation could perhaps be referred to as the rule of 'particular generalisation'.

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[1] See W. V. Quine, *Methods of Logic*, London, 1952, 96

Finally, the unrestricted interpretation in comparison with the restricted one appears to me to be a nearer approximation to ordinary usage. Somehow we do not believe that *everything exists* and we do not see a contradiction in saying that *something does not exist.* It is only from logicians who favour the restricted interpretation that we learn that things are the other way round. We may further add that the unrestricted interpretation of the quantifiers is in complete harmony with the formal quantification theory. I do not know of any formulae which are demonstrable in the formal quantification theory and which, under the unrestricted interpretation, are not applicable to reasoning with empty noun-expressions or do not hold for universes of some specific size.

When we consider (7), (10), (13), (16), and (17) as understood in the light of the restricted interpretation and compare them with their corresponding translations for the purpose of the unrestricted interpretation, i.e. with (7*a*), (10*a*), (13*a*), (16*a*), and (17*a*), we cannot fail to notice that the idea of the restricted quantification is not a simple one. The translations reveal that it can be analysed into two separate constituents: the idea of the unrestricted quantification on the one hand and the notion of existence on the other. In my opinion the most serious disadvantage of the theory of the restricted quantification is that by merging the idea of quantification with the notion of existence it has put logicians and philosophers on a wrong track in their endeavours to elucidate the problem of existence in logic. In what follows we shall adhere to the theory of unrestricted quantification and we shall attack the problem of existence in logic by determining the meaning of the constant term 'exist(s)' as used in the function 'x exist(s)'.

From the logical point of view there are two satisfactory methods of determining the meaning of a constant term. The one consists in setting forth a set of axioms for the term in question. The other is adopted whenever we give a definition of the term in question with the aid of other terms whose meaning has already been determined axiomatically.[1] We may add at once that we shall employ the latter method.

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[1] See J. Łukasiewicz 'The Principle of Individuation', *Proceedings of the Aristotelian Society*, Sup. Vol. 27, London, 1953, 77 sq.

The meaning of ' exist(s) ' can best be determined on the basis of the logic of noun-expressions constructed as a deductive system by Leśniewski in Warsaw in 1920 and called by him ' Ontology '.[1] The original system of Leśniewski's Ontology is based on singular inclusion (a is b or in symbols $a \in b$) as the only primitive function. For various reasons, however, I prefer to continue my analysis of ' exist(s) ' with reference to a system of Ontology based on ordinary inclusion, which I shall write in the following manner :

$$a \subset b \tag{18}$$

I shall read it ' all a is b ' or ' all a's are b's '. I prefer doing this because ordinary inclusion seems to be more intuitive to an English speaking reader than Leśniewski's singular inclusion. Thus for instance ordinary inclusion has recently been used by Woodger in his ' Science without Properties '[2] for the purpose of constructing a language whose general tendency approximates the tendencies embodied in Ontology.

The functor of ordinary inclusion is a proposition forming functor for two arguments either of which is a noun-expression. If in (18) we substitute constant noun-expressions for the variables ' a ' and ' b ' then the result of the substitution will be true if and only if everything named, or designated, by the noun-expression substituted for ' a ' is also named by the noun-expression substituted for ' b '. It may be of some historical interest to mention that the above semantic characterisation of inclusion can be traced back to Hobbes who used it in order to determine the meaning of the copula ' est ' in propositions such as ' homo est animal '.[3]

From the semantic characterisation of inclusion[4] it is evident that the following propositions are true :

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[1] See S. Leśniewski, ' Über die Grundlagen der Ontologie ', *Comptes rendus des séances de la Societé des Sciences et des Lettres de Varsovie*, Classe III, 1930, **23**. For a brief account of Ontology see K. Ajdukiewicz, ' On the Notion of Existence ', *Studia Philosophica*, Posnaniae, 1951, 8 sq., or J. Łukasiewicz, ' The Principle of Individuation ', *Proceedings of the Aristotelian Society*, Supplementary Volume XXVII, London, 1953, 77 sq.

[2] See this *Journal*, 1951, **2**

[3] Hobbes wrote : ' Ut qui dicit *homo est animal* intellegi ita vult ac si dixisset " si quem recte *hominem* dicimus eundem etiam *animal* recte dicimus ".' See ' Leviathan ', *Opera Philosophica*, iii, 497 (Molesworth) ; see also ' De Corpore ', *Opera Philosophica* i, 27 (Molesworth).

[4] Strictly speaking the meaning of inclusion ought to have been determined axiomatically. I understand from Dr B. Sobociński that Leśniewski had an axiom for inclusion. It has never been published and I never saw it when I was studying

$\text{man} \subset \text{animal}$
$\text{man} \subset \text{man}$
$\text{Socrates} \subset \text{man}$
$\text{Socrates} \subset \text{Socrates}$
$\text{Pegasus} \subset \text{animal}$
$\text{Pegasus} \subset \text{Socrates}$
$\text{Pegasus} \subset \text{Pegasus}$

The last three propositions are true because nothing is designated by ‘ Pegasus ’. Thus whatever is designated by ‘ Pegasus ’, is designated by anything you like. The corresponding proposition can be formulated in symbols as follows :

$$(a)(\text{Pegasus} \subset a) \tag{19}$$

If instead of ‘ Pegasus ’ in (19) we write the constant noun-expression ‘ Λ ’, which designates nothing, then we shall get the following ontological thesis :

$$(a)(\Lambda \subset a) \tag{20}$$

‘ Λ ’ can be defined in terms of inclusion but for the sake of simplicity I prefer to introduce it as an undefined term with the aid of (20).

In order to determine the meaning of ‘ exist(s) ‘ we shall need three definitions, which I write below in the form of equivalences :

$$(a)(\text{ex}(a) \equiv (\exists b)(\sim(a \subset b))) \tag{21}$$

$$(a)(\text{sol}(a) \equiv (b, c, d)(\sim(c \subset d) \,.\, (b \subset a) \,.\, (c \subset a) \supset (b \subset c))) \tag{22}$$

$$(a)(\text{ob}(a) \equiv \text{ex}(a) \,.\, \text{sol}\ (a))^1 \tag{23}$$

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with Leśniewski in Warsaw before the war. But I found some time ago that a system of Ontology can be built up on the basis of the following single axiom: $(a, b)\ ((a \subset b) \equiv (c, d)(\sim (c \subset d) \,.\, (c \subset a) \supset (\exists\ e, f)(\sim (e \subset f) \,.\, (e \subset c) \,.\, (e \subset b) \,.\, (g, h, i)(\sim (h \subset i) \,.\, (g \subset e) \,.\, (h \subset e) \supset (g \subset h))))).$

[1] In Leśniewski's original system of Ontology the three functors are defined as follows :

$$(a)(\text{ex}(a) \equiv (\exists b)(b \in a))$$
$$(a)(\text{sol}(a) \equiv (b, c)((b \in a) \,.\, (c \in a) \supset (b \in c)))$$
$$(a)(\text{ob}(a) \equiv (\exists b)(a \in b))$$

See T. Kotarbński, *Elementy teorji poznania, logiki formalnej i metodologji nauk* (Elements of Epistemology, Formal Logic, and Methodology), Lwów, 1929, 235 sq. ; see also K. Ajdukiewicz, ‘ On the Notion of Existence ’, *Studia Philosophica*, Posnaniae, 1951, 4, 8, and J. Łukasiewicz, ‘ The Principle of Individuation ’, *Proceedings of the Aristotelian Society*, Supplementary Volume XXVII, London, 1953, 79 sq.

For the present we need not trouble ourselves with the question how to read the newly defined functors. We can proceed straight on to the consequences which can be deduced from (20) and the three definitions.

Thus from (21) we immediately get

$$\text{ex}(\Lambda) \equiv (\exists a)(\sim(\Lambda \subset a) \tag{24}$$

Since from (20) we know that

$$\sim (\exists a)(\sim (\Lambda \subset a) \tag{25}$$

we use (25) and (24) to show that

$$\sim (\text{ex}(\Lambda)) \tag{26}$$

From (26) we obtain, by particular generalisation,

$$(\exists a)(\sim (\text{ex}(a))) \tag{27}$$

which is equivalent to

$$\sim (a)(\text{ex}(a)) \tag{28}$$

From (23) and (26) we conclude that

$$\sim (\text{ob}(\Lambda)) \tag{29}$$

From (29) we get

$$(\exists a)(\sim (\text{ob}(a))) \tag{30}$$

which is equivalent to

$$\sim (a)(\text{ob}(a)) \tag{31}$$

We can now draw Pegasus into our deductions. From (19) we immediately obtain

$$\sim (\exists a)(\sim (\text{Pegasus} \subset \text{a})) \tag{32}$$

and use it together with (21) to show that

$$\sim (\text{ex}(\text{Pegasus})) \tag{33}$$

From (23) and (33) we derive

$$\sim (\text{ob}(\text{Pegasus})) \tag{34}$$

Now (27), (28), (30), (31), (33), and (34) show that the functors ' ex ' and ' ob ' are very close approximations of ' exist(s) '. We remember that under the unrestricted interpretation of the quantifiers ' $(x)(x$ exists) ' is false and so is ' $(a)(\text{ex}(a))$ ' and ' $(a)(\text{ob}(a))$ ' as is evident from (28) and (31). Under the same interpretation ' $(\exists x)$ $(x$ does not exist) ' comes out true and so does ' $(\exists a)(\sim (\text{ex}(a))$ ' and ' $(\exists a)((\sim (\text{ob}(a))$ ' as is evident from (27) and (30).

Further evidence is supplied by (33) and (34) from which it follows that ' ex(Pegasus) ' and ' ob(Pegasus) ' are false just as ' Pegasus exists ' is admittedly false. It remains then to be explained what is the difference in the meaning of ' ex ' and ' ob '. We can find the required explanation if we consider the meaning of (22), which is a definition of ' sol '. The right hand side of this definition turns out to be true in two cases : if there is no such a thing as *a* or if there is only one *a*. Thus ' sol(*a*) ' can be read as ' there is at most one *a* '. Now if we agree to read ' ex(*a*) ' as ' *a* exists ' or as '*a*'s exist ' then in accordance with (23) ' ob(*a*) ' will have to be read as ' there exists exactly one *a* ', which is equivalent to ' *a* is an object ' or to ' *a* is an individual '. Thus the distinction between ' ex ' and ' ob ' roughly corresponds to the one made by Quine in his ' Designation and Existence ', where he talks about *general* existence statements and *singular* existence statements.[1]

We have already remarked that under the restricted interpretation every component of quantificational expansions contains a noun-expression which designates only one of the objects belonging to the universe. It therefore follows that the function ' *x* exists ' as used by us when we discussed the two interpretations of the quantifiers, means in fact the same as ' there exists exactly one *x* ' which is the rendering of the symbolic ' ob(*x*) '. The functor ' ex ', on the other hand, appears to be a very close approximation of the ' exist(s) ' as used in ordinary language as it forms true propositions with noun-expressions which may designate more objects than one.

I wish to conclude with a brief summary of the results. The aim of the paper was to analyse rather than criticise. I started by examining two inferences which appeared to disprove the validity of the rules of universal instantiation and existential generalisation in application to reasoning with empty noun-expressions. Then I distinguished two different interpretations of the quantifiers and argued that under what I called the unrestricted interpretation the two inferences were correct. Further arguments in favour of the unrestricted interpretation of the quantifiers were brought in, and in particular it was found that by adopting the unrestricted interpretation it was possible to separate the notion of existence from the idea of quantification. With the aid of the functor of inclusion two functors were defined of which one expressed the notion of existence as underlying the theory of restricted quantification while the other approximated the term ' exist(s) ' as used in ordinary language.

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[1] See W. V. Quine, ' Designation and Existence ', in *Readings in Philosophical Analysis*, edited by H. Feigl and W. Sellars, New York, 1949, 44 sq.

It may be useful to supplement this summary by indicating some aspects of the problem of existence which have not been included in the discussion. I analysed the theory of quantification so far as it was applied in connection with variables for which noun-expressions could be substituted and my enquiry into the meaning of ' exist(s) ' was limited to cases where this functor was used with noun-expressions designating concrete objects or with noun-expressions that were empty. It remains to explore, among other things, in what sense the quantifiers can be used to bind predicate variables and what we mean when we say that colours exist or that numbers exist. These are far more difficult problems, which may call for a separate paper or rather for a number of separate papers.

12 Queen's Gardens
London, W. 2

ANALYSIS OF PROJECTION*

J. R. SMYTHIES

THE PURPOSE of this paper is to analyse the concept of ' projection ' as this is currently used in the neurobiological sciences. I shall argue that this term is used (1) in the neurophysiological theory of perception to describe a mythical process; (2) in psycho-analysis; and (3) in neuro-anatomy to describe real processes for which, however, better terms are suggested.

I

In the current neurophysiological theory of (visual) perception physical stimuli in the form of light waves and nerve impulses are supposed to stream continually from external physical objects through the eye, optic tracts, etc., to the occipital cortex. Here complex electronic changes take place and then *something* (it is not clear exactly what) is projected out of the physical head of the individual, out into external space, retracing the path taken by the original stimuli until the final result is that the visual sense-datum is sensed located in its familiar position in the ' external space ' of the visual field, for instance, this little square □ in your own visual field in your very own ' there ' and ' now '. Ruch[1] describes this as follows :

> All our sensations are aroused directly in the brain, but in no case are we conscious of this. On the contrary, our sensations are projected either to the exterior of the body or to some peripheral organ in the body, i.e. to the place where experience has taught us that the acting stimulus arises. The exteroceptive sensations are therefore projected exterior to our body. Sound seems to come from the bell, light from the lamp, etc. . . . An important aspect of sensation which deserves to be called *the law of projection* is that stimulation of a sensory system at any point central to the sense organs gives rise to a sensation which is projected to the periphery and not to the point of stimulation.

Since the final result of this process is evidently a spatial and organised entity (sensum or percept) it would seem a necessary condition for the validity of this theory that some spatial process of the organism would mediate this most complex process of projection. It is notorious that nothing like this has ever been detected. In fact, as Brain[1] has argued, it is very hard to believe that such a process actually occurs. The chief objections that can be laid against the postulate that there is such a process of ' projection ' of sensations are as follows :

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* Received 16. vii. 53. Read to a seminar group in the Department of Philosophy and Psychology, University of British Columbia, April 8th 1954.

[1] See J. F. Fulton, *Textbook of Physiology*, 16th ed., Philadelphia, 1950, 311

(1) It appears to have no basis in physics. No physical entity is projected and this ' projection ' is not a physical process. ' Projection ' should entail the movement in space of an object or entity. If Ruch's ' sensation ' (which is alleged to be projected) is a physiological entity or process it should be constructed of neuronal impulses (in however complex combinations) or some activity (or even inactivity) of nerve cells and fibres. Neither nerve cells nor fibres, neuronal impulses, atoms, molecules, radiation, nor any chemical, electrical or physical activity of any kind is projected from the cerebral cortex into the external world, except some minute electrical fields which no-one has ever suggested mediate the alleged ' projection ' of sensations. What else physical is there to project ? No-one has ever detected or measured this process in the laboratory. Since it is not a physiological process at all what possible status could it have ?

(2) Ruch's introduction of the concept of learning to account for the phenomenon implies that, in the infant, sensa are actually in the brain but at some time during development they leave the brain and get projected into external space. This merely postpones the development of the process which has never been empirically detected.

Now it might be admitted that nothing physical ever gets projected from the cerebral cortex back into the external world. In that case could we say that ' projection ' means that the brain ' knows ' that physical objects are spatially external to its own physical organism—using ' know ' in the sense of having gleaned a basic operational rule or item of information from its past working just as a computing machine might extract such an item of information out of the total complex of information fed into it during its working life ? But this would not explain the basic fact that the *end results* of the physiological processes of perception are outside—spatially outside—the physical organism. For is it not a certain fact of direct experience that the coloured and expanded patches[2] which I experience directly are spatially external to this my physical organism which I also experience directly as my own body ? How then did these coloured patches get outside my body ? 'Externality is the cardinal problem.'[1] Two ways out of this dilemma suggest themselves :

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[1] W. Russell Brain, *Mind, Perception and Science*, Oxford, 1951

[2] i.e. ' percepts ' or ' sensa '. Both these terms will be taken to stand for the distinguishable elements of experience—colour patches, sounds, smells, etc.

(i) Some of the entities named above may have been misidentified, or (ii) percepts (sensa) are not the final products of the physiological processes of perception. It will be convenient to discuss (i) first for this leads directly to the third argument against 'projection'.

(3) It may be argued that it is superfluous to postulate a process of 'projection' since a proper attention to the internal detail of the physiological theory itself eliminates the necessity for making it. For if we admit the physiological account of the process of perception (which account led in the first place to the postulation of 'projection') then somatic sensa should be treated in a similar way to visual sensa. If the latter are the final products of a mechanical process of 'unconscious reception, selection, differentiation, integration and interpretation of sensory stimuli'[2] then so must the former.[3] I suggested above that it is self-evident that visual percepts are spatially external to the physical organism. This, however, is false. What actually is evident in experience is that visual sensa (percepts) are spatially external to what is usually loosely called 'my (human) body' (or in neurology the 'body-image') but which should strictly be called the 'sensed body' or 'somatic sensory field'.[4] This is

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[1] Brain, loc. cit.

[2] William Goody and Margaret Reinhold, *Brain*, 1952, 75, 472

[3] There is no essential difference between the neuronal activity mediating vision and that mediating somatic sensation

[4] The term 'body-image' is used in neurology loosely to stand for both the *body-concept* (the system of conceptual knowledge, belief and attitude that we each possess about our own bodies) and the *sensed body*. A clear instance of the latter use may be found in Macdonald Critchley :

'In such conditions (in cases where there was an imperception for one half of the body), the most plausible explanation of these cases of tactile allochiria is to imagine a unilateral defect of the body-image, without, however, absolute sensory loss. A pinprick applied to the affected side is felt, but it is not acccurately projected, that particular half of the body-image being in abeyance. It is therefore naturally transferred to a corresponding point on the intact half of the body-image' (p. 138). And again :

'With the onset of this vision, the feeling of strangeness of the left half of his body-image disappeared' (p. 240).

Critchley had previously defined the body-image as '. . . the mental idea which an individual possesses as to his body and its physical and aesthetic attributes' merely the collection of all somatic sensa (touch, pain, thermal, pressure ; skin, muscle, bone, joint and visceral sensa) within the field of direct experience during any one specious present. We may say that visual sensa are external to somatic sensa in perceptual space. If the current version of the physiological theory be accepted this relation must be identified (in physical space) not with the relation of externality which surrounding physical objects bear to the physical (human) body, but with the relation in space that the parietal lobe bears to the occipital lobe. For *if* visual sensa are identified with events in the occipital cortex (or cortico-thalamic connections) then somatic sensa should be identified with Penfield's [1] 'sensory homunculus' in the parietal cortex (or cortico-thalamic connections). Thus no question of projecting anything arises. If we do not accept the physiological account then there is no need to talk about projection either, for visual sensa may then be supposed to be external physical objects and the somatic sensory field may be supposed to be the physical body. This is incompatible with the physiological account of perception, which loses all reference to the everyday processes of seeing, hearing, etc. This may be considered a steep price to pay to bolster naïve realism but it seems to be logically tenable. But it does not seem sound practice to treat visual sensa in a physiological way and somatic sensa in a naïvely realistic way.

(4) Brain's argument is as follows :

> Thus when we perceive a two-dimensional circle we do so by means of an activity in the brain which is halved, reduplicated, transposed, inverted, distorted, and three-dimensional. If physiological idealism is to be really physiological it must admit that its theory of projection breaks down because the circle which is said to be projected from the cerebral cortex never existed there at all.[2]

In some neurophysiological theories of perception the final processes concerned are supposed to be effected by electrical switch scanning mechanisms present in the cortex which are supposed to

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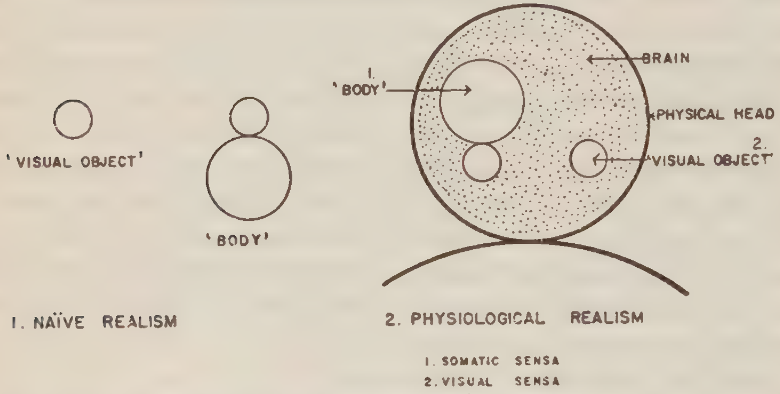
(i.e. the body-concept). (*Lancet*, 25-ii-50, p. 335.) But a mental idea does not have spatial halves, nor is it concerned with sensory localisation, nor can an idea *feel* strange. 'Body-image' here clearly refers to the somatic sensory field. See Macdonald Critchley, *The Parietal Lobes*, London, 1953 and J. R. Smythies, *Brain*, 1953, **76**, 132.

[1] Wilder Penfield and A. T. Rasmussen, *The Cerebral Cortex of Man*, New York, 1950

[2] Brain, loc. cit., 9

work in a manner similar to television. However, the end result of the television process consists of the pictures or images on the television screen and these are not identical with the actors and scenery in the television studio. Such pictures are nowhere except on the television screen and are mechanical constructions of the television mechanism. Similarly if visual sensa are held to be the final product of the neuro-visual mechanism then they would be nowhere except in the cortex and would be mechanical constructs of the cortical mechanisms. Thus spatially *behind* visual sensa and *around* somatic sensa would lie more brain tissue : sensa themselves would be electrical patterns of neuronal excitation carried on the nerve nets of the brain. So, if somatic sensa are given the same status as visual sensa, then sensa could literally be in the brain, just as television images are in the television screen and Russell's elaborate alternative meanings for this relation of ' in ' become unnecessary. Sensa are intrinsically spatial entities—the following statement is certainly true, ' visual sensa and somatic sensa are extended in space '. As such they may bear a spatial relation of some kind to the brain as an expanded physical object. It is possible that this relation is that of being ' inside ' in a spatial sense. We thus avoid unobservable, hence mythical, process of ' projection '.



This diagram shows the different relationships postulated by common sense and physiology between the elements of experience and the brain.

Thus a strict statement of the physiological theory of perception, couched in terms of the sense-datum analysis, would seem to lead us to locate sense-data spatially in the brain (using ' in ' in the sense given above), that is, if we deny both the ' projection ' of sensations and transactions with an unextended ' mind ', both of which processes are undetectable or at any rate have never been detected. But can this theory be maintained ?

It may be objected that sensa cannot be thus located, for if they were, then a neurosurgeon cutting open a living brain in a conscious subject should be able to find them, which, it may be argued, he does not. Furthermore it seems equally certain, if we consider the anatomy of the brain, that sense-data cannot literally be parts of the brain, for the same argument that Brain uses against projection applies equally cogently against the theory of the cerebral localisation of sense-data. For the essential function of the brain is to construct spatio-temporal patterns of neuronal impulses—Brain's ' schemata ' : sense-data are also spatio-temporal entities and the two [1]—as Brain points out—are spatio-temporally quite incongruous and cannot be *identical*. But is there any necessity to use the sense-datum analysis when discussing the complete physiological theory of perception (i.e. the theory designed to account for the total process of perception and not merely the detail of events in small parts of the visual mechanism) ? Could we not use the fashionable ' common-sensical ' analysis as presented recently, for example, by Martin Lean ?[2] This would be to follow the second way out of our dilemma[3] suggested above. In spite of Lean's claim that, in its account of perception, physiology is not against common sense I do not think that this can be done for the following reasons :

(i) There are the difficulties of the finite velocity of light, the occurrence of double vision, and illusions and hallucinations of all kinds which are sufficiently well-known not to need elaboration here. Lean claims that the explanation for all these difficulties (except the first) is to be found in the principles of optical perspective ' physics and physiological psychology : and it is not contrary to common sense '. But against this the following may be urged :

(ii) The current neurophysiological theory of perception actually belongs to the class of representative theories ; it is what Lean himself calls a ' television ' theory of perception. There is a good deal of empirical evidence in neurology and experimental psychology which seems to show that the visual field in direct experience is actually constructed by a mechanism which shares many features in common with the familiar television mechanism. 'In other words, the televisual system behaves very much like the neuro-visual one.'[1]

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[1] i.e. the sense-datum and the schema belonging to any one object perceived by any one person during any one specious present

[2] Martin Lean, *Sense Perception and Matter*, London, 1953

[3] i.e. that percepts (sensa) are not the final products of the physiological processes of perception

We know the stroboscopic patterns—the complex sensory patterns witnessed on looking at a flashing electronic stroboscope—show the actual detail and execution of this construction. This evidence in neurology[2] includes the phenomena of micropsia, macropsia, teleopsia, metamorphopsia, inverted vision, space and film colours, the various forms of visual perseveration and disorders in the spatial relations of sensa to each other in the visual field, optic allo-aesthesia, etc. We have examples from experimental psychology which seem to support the contention that the visual field in direct experience is built up by a television-like mechanism, are the 'constant-size' effect—which shows that distant vision does not obey the physical rules of perspective—and the phenomenon known as Fuchsian completions : in this, if an incomplete though nearly complete circle or other regular simple figure is presented to a subject under certain conditions the *complete* figure will be *sensed.* This evidence shows that, in the perception of people with damaged brains and people in special perceptual situations, sense-data cannot be parts of external physical objects. Since there is always continuity between normal and abnormal sensation how can we then say that this can be true for normal perception either ? Since all this mass of empirical evidence can only obtain a plain explanation in terms of the representative theory (while the important stroboscopic phenomena are quite unintelligible on any other theory) one wonders why this theory is so unfashionable among contemporary philosophers. Perhaps we can say that if we want to construct a comprehensive theory of perception it may be advisable to study the perceptual processes themselves rather than confine ourselves to a study of ordinary language. Lean's claim—that the current physiological theory of perception would support his own theory—may be denied for it is clear that the 'television' theory is quite at variance with 'common sense'.

Thus naïve realism and physiological realism seem, in the light of the evidence, to be equally unsatisfactory. In view of the difficulty of finding an adequate theory of perception we must be prepared to reject false theories even if we have not the faintest idea what the true theory might be. However, there is one more theory to be considered —that which suggests that sense-data are in the mind. But what could be meant by saying that sense-data are ' in the mind ' ? Usually such a statement leads to a thorough-going idealism, but recently two different methods have been developed which interpret this statement realistically. Price has recently presented a theory of the ontological status of images [1] which may be summarised thus : (i) Images are in a space of their own which space is a different space from physical space : and (ii) images (themselves spatial entities) bear *causal* relations but no *spatial* relations to physical objects and events. Now this theory of images implies a theory about sense-data since it is possible to show empirically that the sense-data and images of one individual are in one and the same space—that is do bear spatial, or spatio-temporal, relations to each other. For direct spatio-temporal continuity may be observed between sense-data and images. Sense-data are spatio-temporally continuous with their after-sensations. Under mescal after-sensations may be observed to turn directly into typical mescal images. These latter are a highly developed form of hypnagogic images which in turn merge imperceptibly into ordinary visual images. Thus we can develop Price's theory to suggest : (iii) one man's sense-data and images are in a (three-dimensional) space of their own (which Russell calls ' private ' space—ontologically and geographically a *different* space from physical space) and they bear causal relations but no spatial relations to physical objects and events. Thus there is not *one* space-time but many different space-times. There is one common physical space-time and many private space-times. Between physical objects and sense-data (and images) there are causal relations but no spatial relations (i.e. none of these space systems bears any *spatial* relation to any other) : while physical objects bear both spatial and causal relations to other physical objects, sense-data bear spatial but probably not causal relations to other sense-data.

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[1] W. Grey Walter in *Perspectives in Neuropsychiatry*, London, 1950

[2] See the writings of Klüver, Schilder, Sir Russell Brain, Macdonald Critchley, Bender, and others

The second way of saying that sense-data are ' in the mind ' in a realistic manner—theory (iv)—is quite similar to theory (iii) except that it suggests that sense-data and images do not bear *no* spatial relations to physical objects and events but bear rather higher-dimensional spatial relations to them. In other words, the one public physical space-time and the many private space-times *are* spatially related (as well as their contents being causally related) and form, for n individuals, a $(3n + 3)$ dimensional manifold.

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[1] H. H. Price, ' Survival and the Idea of " Another World " ', *Proc. Soc. Psychical Research*, 1953, **50**, 1-25

Both these theories are realist theories—i.e. they deal only with spatio-temporal entities—and, in as far as they are theories of perception, they are representative theories. They have all the interesting and important bearing on the theory of mind and mind-brain relation that Price's original theory has.

2

In psycho-analytical theory the concept of ' projection ' is used primarily to describe the process whereby our own feelings and attitudes, of which we are ashamed or which we find painful and to which we do not wish to admit, are ascribed to others. We come to believe that these other people actually entertain these same feelings towards us and our behaviour is adjusted accordingly. Freud[1] describes the process thus with respect to the primitive's feeling of hostility toward the recently dead : (i) ' This unknown hostility, of which we are ignorant and of which we do not wish to know, is *projected from our inner perception into the outer world* and is thereby detached from our own person and attributed to the other.' And again he talks of (ii) : ' the tendency to *project psychic processes into the outer world.*' Fenichel[2] says : (iii) ' Affects may be projected, that is, *perceived in someone else to avoid perceiving them in oneself.*' Alexander[3] says : (iv) ' When the ego can no longer keep objectionable tendencies out of consciousness, more drastic defences become necessary. One of these more radical measures is to attribute repressed tendencies and desires to others.' In the glossary to this work ' projection ' is defined as (v) ' The ascription to the outer world of mental processes that are not recognised to be of personal origin.'

Now in examples (i) and (iii) above the actual meaning is ambiguous owing to the use of the word ' perception '. In examples (iv) and (v) the meaning is clear as the word is not used. ' Perception ' has a number of meanings in common use and as a technical term in philosophy. In philosophy it may be used to mean the actual basic process of being aware of, or sensing, sense-data, hallucinatory or

[1] Freud, *Totem and Tabu* (in Brill : see below. My italics)

[2] O. Fenichel, *The Psychoanalytical Theory of Neurosis*, New York, 1945 (my italics)

[3] F. Alexander, *Fundamentals of Psychoanalysis*, New York, 1938

'real'. No questions of recognition or belief are involved. There is just a Self and a sense-datum in the relationship called perceiving of the latter by the former: a better word is sensing (Price[1]). This is roughly the process which is left relatively intact in object-agnosia. In common use the meaning of 'to perceive' may best be illustrated by examples: 'He perceived that he was alone in the room' or in Fenichel's phrase quoted above, 'Affects may be projected, that is, perceived in someone else, . . .' This is clearly a much more complex process than the former. It involves the sensing of a sense-datum or collections of sense-data; the *perceptual assurance* (Price[2]) that material objects exist to which these sense-data belong; and the understanding of the complex meaning of the total situation and the formation of beliefs in various degrees of complexity. In examples (i) to (iii) above it is, in fact, clear that it is this latter sense of perception that is meant. The slight ambiguity aroused by the fact that perception is also used in other and technical ways in philosophy, one of which is given above, would not be of any importance were it not for the fact that 'projection' is also used to account for the genesis of hallucinations, which is a very different matter indeed.

If one talks of 'projecting affects', 'projection', as Brain[3] has pointed out, implies actual movement in space of an object. Clearly, in the process under discussion, no such movement takes place. Little pieces of affect are not projected out of my body and alighting on your body are there sensed or perceived by me. This is absurd and was obviously never meant by Freud although it represents the proper meaning of the words used. Clearly, therefore, they are being used as an analogy. Why then, one wonders, was it necessary for Freud to use such a clumsy analogy when perfectly good terms were at hand to describe the whole process in literal terms? For instance, 'ascription' is a much better word (actually used by Alexander: example (v) above) since it implies only the changes in *belief* and *attitude* that actually occur. The reason is that Freud was trying to include two basically different processes under one term—i.e. the *genesis of hallucinations* and the *ascription of affects*. According to Brill,[4] (vi): '. . . we know from Freud that hallucinations represent *outward projections of inner feelings*'. And similarly Fenichel[5] (vii):

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[1] H. H. Price, *Perception*, London, 1950

[2] Price, loc. cit.

[3] Brain, loc. cit.

[4] A. A. Brill, Introduction to *The Basic Writings of Sigmund Freud*, New York, 1938

[5] Fenichel, loc. cit.

' Projection, by *putting unpleasant sensations into the external world, . . .*' Here clearly we are dealing with something different from the unconscious ascription of affects: we are dealing with sensory phenomena. Hallucinations are *particular existents* (Price[1]) and are quite different from beliefs—they are entities about which we hold beliefs. They are in fact ' wild ' sense-data and cannot be projected from the brain any more than normal sense-data can.

I am not denying that the process referred to by analysts as ' projection ' may occur exactly as they describe it or that ' projection ' is a good analogical term for it. But analogical terms can be dangerous and it is better to use literal terms when one can. However, I do not suppose that it would be possible to dissuade analysts from using ' projection ' as a technical term—it is too set into the whole structure of psycho-analysis for that. My hope rather is that when people use psycho-analytical material in or for philosophical enquiry or analysis and read about ' projection ' of affects, wishes, attitudes and feelings, they will say to themselves, ' ascription really,' and when they read about the ' projection ' of hallucinations they will say, ' What is meant here is that the *occurrence* and *form* of hallucinations are *determined* by the wishes and emotions of the subject according to the " laws " of psycho-analysis. Nothing is really projected *in a literal sense*.'

Emotions have undeniable effects on the content of hallucinations but they produce their effects wholly by means of the internal cerebral mechanisms of the organism (probably through the mechanism described by Papez),[2] and no process of projection ever occurs, although various complex changes of unconscious ascription do occur (such as have been investigated experimentally by Morton Prince[3]).

The word ' projection ' has also been used to describe the alleged projection of mental images into outer space. For instance, Goody and Reinhold[4] talk of ' the individual's ability to project a mental image of himself into space ', and again, ' . . . the individual is able to project a mental image of himself, by means of abstract thought processes, into space '. As in the case of sense-data this process of the projection of mental images is a mythical one. Mental images are always where they are experienced in their own field. This field is also inside the organism.

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[1] Price, loc. cit.

[2] W. Papez, *Arch. Neurol. Psych.*, 1937, **28**, 725

[3] M. Prince, ibid., 1922, **7**, 780

[4] W. Goody and M. Reinhold, loc. cit.

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‘ Projection ’ is used in yet a third sense in the biological sciences—in neuro-anatomy. A nucleus in the thalamus, for instance, may be described as projecting to a certain part of the cortex or the motor cortex is described as projecting to the anterior horn cells of the spinal cord. This merely means that experiments have shown that an anatomical and physiological connection exists between the two. Cells in one place give off axons which may be traced to the other place and there synapse with other cells. There is a material causal link between the two just as there are wires between my electrical light bulb and the power station. But again no object is literally projected. A nerve impulse is not an object but is a wave of activity in an object. Thus we should really say ‘ the thalamus *transmits* to the cortex ’. If asked ‘ What is transmitted? ’ we can answer ‘ nerve impulses and these transmitted impulses are *conducted* by nerve fibres.’ But this is really a fine distinction and the use of ‘ projection ’ is much less objectionable in this sense than it is in the ‘ projection ’ of sensations. For there are spatial structures and causal processes linking the thalamus and the cortex (which are both spatial entities), while there are no demonstrable spatial structures nor causal processes to link the brain and sense-data (which are both also spatial entities), nor to execute this ‘ projection ’, if the latter are regarded as being projected into the external physical world. We cannot talk of sensations (or sensa) being ‘ transmitted ’ nor ‘ conducted ’ into the external physical world and this gives us further insight into the mythical character of the ‘ projection ’ of sensations.

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Thus when we are talking about our beliefs as to the state of other people’s feelings about us we should use the term ‘ ascription ’. This covers the bulk of its use in psycho-analysis. If we are talking about visual hallucinations (or normal sensing) such sense-data have their actually experienced positions in the visual field which is the sensed part of the signalling mechanism connecting with the external physical world. Hallucinations (and normal sense-data) are processes and structures *inside* the human organism. They arise inside it and stay there and are never projected into the outer world at all. They are no more in the outer world than the images on the television screen are literally in the television studio. The confusion, to repeat, has arisen because the visual sense-data and the field made up by them are certainly outside the *sensed body* and as long as this latter is identified with the physical organism it becomes necessary to invent processes whereby visual sense-data should also be geographically outside the physical organism. Thus the spatial relation that actually holds between the visual field and the somatic sensory field is dislocated to describe a spatial relation between visual sense-data and the physical organism. So, while visual sense-data are undoubtedly outside the somatic sensory field and external physical objects are outside the physical organism—while these *relations* are the same—the entities bearing them are extended in different space systems, in perceptual space and physical space respectively. Visual and other sense-data do *not* lie outside the human organism. Nor can they be identified with any physical objects but, as the evidence in neurology seems to suggest, they may be connected to them by a signalling mechanism. These points are made clearly by Bertrand Russell :[1]

> The table as a physical object, consisting of electrons, positrons, and neutrons, lies outside my experience, and if there is a space which contains both it and my perceptual space, then in that space the physical table must be wholly external to my perceptual space. . . .
>
> All this, I say, has long been a commonplace, but it has a consequence that has not been adequately recognised, namely, the space in which the physical table is located must also be different from the space that we know by experience.

Köhler [2] writes in the same vein :

> I learned that physical objects influence a particularly interesting physical system, my organism [physical body] and that my objective experience results when, as a consequence, certain complicated processes have happened in this system. Obviously, I realised, I cannot identify the final products, the things and events of my experience [sense-data], with the physical objects from which the influences come. If a wound is not the gun which emitted the projectile, then the things which I have before me, which I see and feel, cannot be identical with the corresponding physical objects. These objects merely establish certain alterations within my physical organism [physical body], and the final products of these alterations are the things which I behold in my visual field [visual sense-data], or which I feel with my fingers [tactual sense-data]. We have seen the same warning applies to the relation between my organism as a physical system and my body as a perceptual fact [sensed body or somatic sensory field ; usually miscalled ' body-

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[1] Russell, *Human Knowledge*, London, 1948, 236, 238

[2] Wolfgang Köhler, *Gestalt Psychology*, New York, 1947 (my italics)

> image ']. My body [sensed body] is the outcome of certain processes in my physical organism. Processes which start in the eyes, muscles, skin and so forth, exactly as the chair before me [visual sense-data *belonging* to the chair] is the final product of other processes in the same physical organism. *If the chair is seen ' before me ', the ' me ' of this phrase means my body as an experience, of course, not my organism as an object of the physical world.* Even psychologists do not seem to be entirely clear about this point.

The confusion between the somatic sensory field and the physical organism may lie at the basis of many of the contemporary problems of mind.

Summary

The use of the term ' projection ' should be abandoned in the psychophysiology of perception. It has been used to describe the alleged projection of sensations (or sensa), including hallucinations, out of the brain of the individual into the external physical world. Since this process is a mythical or unobservable one with no demonstrable physiological basis it should have no place in natural science. As an alleged process in the physiology of perception it may be that it has played a part similar to that once played by phlogiston in chemistry.

This is well stated by Russell :[1]

> Whoever accepts the causal theory of perception is compelled to conclude that percepts are in our heads, for they come at the end of a causal chain of physical events leading, spatially, from the object to the brain of the percipient. We cannot suppose that, at the end of this process, the last effect suddenly jumps back to the starting point like a stretched rope when it snaps.

Furthermore, if there is really no such process of projection then the physiological theory of perception becomes untenable in its present form and this paper presents two new theories of perception designed to avoid these difficulties.

In psycho-analysis ' projection ' has been used as a term to describe the ascription of one's own repressed affects and attitudes to other people. In this case ' ascription ' is a better term.

In neuro-anatomy ' transmission ' should be used instead of ' projection '.

Department of Neurological Research
University of British Columbia
Vancouver
Canada

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[1] Russell, *The Analysis of Matter*, London, 1927

ON VARIABLES IN MATHEMATICS AND IN NATURAL SCIENCE*

KARL MENGER

IN the development of mathematics and natural science which began in the seventeenth century, perhaps the most characteristic concept is that of a variable. But what is a variable ? Russell, who investigated all aspects of this notion, called it ' certainly . . . one of the most difficult to understand ' and in 1903 stated that a satisfactory theory of variables was not to be found in his *Principles of Mathematics*. In fifty years this situation has not been improved.

NUMERICAL VARIABLES AND VARIABLES IN THE SENSE OF THE LOGICIANS. Let C be a class of numbers.[1] Weierstrass defined a *numerical variable* or, more specifically, a *variable with the range* C as a symbol standing for any element in C. The letter ' x ' in $D \log x = 1/x$ (where D denotes the derivative) is a variable of which the range is the class of all positive numbers. So is the letter ' y ' in $D \log y = 1/y$. Logic deals with a generalisation of Weierstrass's concept. The logicians admit any class as the range of a variable ; for example, the letter ' f ' in the formula $Df^2(0) = 2f(0) \cdot Df(0)$ is a variable of which the range is the class of all functions which are differentiable at 0. (Here the number 0 can, of course, be replaced by any other number.) Natural science, on the other hand, deals with variables such as gas pressure and distance travelled by a falling object, which have no direct relation to Weierstrass's concept.

VARIABLE QUANTITIES. In better agreement with scientific usage, French analysts have defined a variable as ' une quantité qui passe par une infinité de valeurs réelles, distinctes ou non '. However, this definition has never been supplemented either by explicit definitions of ' quantité ' and ' passer par ' or by postulates which implicitly define these concepts, as postulational geometry defines ' points ' and ' lines '. An attempt is here made to fill this gap with a theory [2] of variable

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* Received 7. x. 53

[1] In this paper, number means finite real number

[2] cf. the author's note ' The Ideas of Variable and Function ', *Proc. Nat. Acad. Sci.*, 1953, **39**, 956–961, and the second edition of his book, *Calculus : A Modern Approach*, Chicago, 1953

quantities—briefly, v.q.'s. Whether or not v.q.'s are considered to complete the French notion, they are to be clearly distinguished from variables in the sense of either Weierstrass or the logicians.

Let A be any class. By a *v.q. with the domain A* we mean[1] a class of pairs such that (1) in each pair the first member is an element of A, and the second a number (called a *value* of the v.q.), (2) each element of A is the first member of exactly one pair belonging to the class. The class of all values is called the *range* of the v.q. Let u be a v.q. with the domain A, and (α, n) a pair belonging to u. Then α is an element of A, and n is a number. By (2), u does not contain a pair (α, n') with $n' \neq n$; hence n is called *the value of* u *for* α or $u\alpha$. Consequently, u is the class of all pairs $(\alpha, u\alpha)$, where α is any element of A. (Thus, in the sense of logic, α is a variable with the range A.)

If K is the class of all circles in a plane, and $r\kappa$ denotes the radius of the circle κ, then the class r of all pairs $(\kappa, r\kappa)$ is a v.q. with the domain K. If $a\kappa$ denotes the area of κ, the class a of all pairs $(\kappa, a\kappa)$ is another v.q. with the domain K. The class K falls into disjoint subclasses, each containing all circles which are congruent to any circle of that subclass. If $\kappa \cong \kappa'$, then $r\kappa = r\kappa'$ and $a\kappa = a\kappa'$. We say that variables r and a are invariant with respect to congruence.

SCIENTIFIC V.Q.'s. Let Γ be the class of all acts of reading a manometer connected with any 1 mole sample of gas at a temperature of 100°. Non-simultaneous acts (even if performed on the same sample) are regarded as distinct. For each act γ belonging to Γ, let $p\gamma$ denote the result of the act, i.e. the observed number of units of pressure. The class p of all pairs $(\gamma, p\gamma)$ for any γ belonging to Γ is a v.q. with the domain Γ, called *the pressure of* 1 *mole of gas at* 100°. If Γ' is the class of volumetric observations made on any 1 mole sample of gas at 100° and if, for any γ' belonging to Γ', $v\gamma'$ denotes the result of γ', then the class v of all pairs $(\gamma', v\gamma')$ is a v.q. with the domain Γ', called *the volume of* 1 *mole of gas at* 100°. Other examples of v.q.'s are connected with objects dropped from the zero point of a vertical scale. If, for any act σ by which the scale mark opposite a falling object is observed, $s\sigma$ denotes the mark observed, then the class s of all pairs $(\sigma, s\sigma)$ is a v.q., called *the distance travelled by any falling object.* Let t be the class of all pairs in which the first member is an act of reading a timer, set in motion when an object is dropped from the zero point of the scale, and the second member is the result of this reading. Here t is a v.q. called *the time elapsed since the release of any falling object.* The class T of all first members is the domain of t. If τ denotes any element of T (and thus, in the sense of logic, is a variable with the range T), t is the class of all pairs $(\tau, t\tau)$. Any number $t\tau$ is called a value of t.

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[1] A v.q. with the domain A in the sense of this definition is what might be called a function defined on A. This paper follows the usage of the physicists in restricting the concept of a function to the cases where the domains consist of numbers.

A v.q. the range of which consists of only one number is called *constant.* For instance, while a car is parked, the mileage travelled and the speed are constant v.q.'s.

The domain of any one of these v.q.'s, say Γ, falls into mutually disjoint subclasses, each containing all acts simultaneously performed on the same sample by different observers. If two such acts are called parallel, p is, at least in a first approximation, invariant with respect to this parallelism. The domains of other v.q.'s (e.g. temperature) comprise acts of observation on different instruments (mercury thermometers, alcohol thermometers, thermo-couples, etc.) and even what P. W. Bridgman calls ' paper-and-pencil-operations '.

FUNCTIONS. A v.q. the domain of which is a class of numbers is called a *function.* Examples are : (1) the logarithm of any positive number ; (2) the reciprocal of any number other than zero ; (3) 16 times the square of any number ; (4) a constant function, e.g. that with the value 16 for any number. These functions are respectively the classes of all pairs (1) $(x, \log x)$ for any positive x ; (2) $(x, 1/x)$ for any $x \neq 0$; (3) $(x, 16\,x^2)$ for any x ; (4) $(x, 16)$ for any x. In these definitions, x is a numerical variable and may be replaced by any other symbol ; e.g. the function (1) is the class of all pairs $(y, \log y)$ for any positive y.

A natural symbol for the function (1) is ' log '. The functions (2), (3), and (4) have no traditional symbols. If n is any integer, by I^n we denote the nth power function, i.e. the class of all pairs (x, x^n) for any x if $n \geqq 0$, and for any $x \neq 0$ if $n < 0$. The value of I^n for x (that is, x^n) can also be written $I^n x$. Function (2) is I^{-1}. We denote (4) by ' $\underline{\underline{16}}$ ', and (3) by ' $\underline{\underline{16}}.I^2$ '. Even the ' identity ' function I, the class of all pairs (x, x), in spite of its paramount importance, has no standard symbol in classical analysis. This situation is reminiscent of the lack of a symbol for the number zero in Greek arithmetic, and results in similar complications. Traditionally, I^n is referred to as the function x^n (I, as the function x) and, for the sake of uniformity, every function is named after its value for x. For instance, the function $\underline{\underline{16}}$ is denoted by ' 16 ' (a practice which invites confusion with the number 16) and, although no one speaks of log x tables, the function log is referred to as the function log x—a strange *pars pro toto*, since the symbol of the value is longer than the available symbol of the function itself.

Let f be any function, that is, a class of pairs of numbers (x, y) where no two pairs have equal first and unequal second members.[1] The domain of f (briefly, dom f) is the class of the first members of the pairs. The value of f for x may be denoted by $f(x)$—briefly, fx, just as the value of log for x is denoted by log x rather than by log(x). If u is a v.q. the range of which is a part of dom f, and if α is any element of the domain A of u, then $u\alpha$ belongs to dom f, and the value of f for $u\alpha$ is $f(u\alpha)$—briefly $fu\alpha$. The class of all pairs $(\alpha, fu\alpha)$ for any α belonging to A is a v.q., denoted by fu. It is called the result of *substitution* of u into f. In particular, a function g the range of which is a part of dom f can be substituted into f; the result is a function, fg, having the same domain as g. (N.B. Juxtaposition, as in I^2I^3 $(= I^6)$, denotes substitution, not multiplication. The product of f and g will be indicated by $f{\cdot}g$, for which $\underline{16}.I^2$ and $I^2{\cdot}I^3$ $(= I^5)$ are examples.)

No v.q.'s but functions[2] permit the substitution of other v.q.'s. Any function f certainly permits the substitution of the function I', defined as the class of all pairs (x, x) for any x belonging to dom f.[3] Into the function I, every v.q. can be substituted, and $Iu = u$.

That among the v.q.'s the functions are characterised by the fact that they lend themselves to the substitution of other v.q.'s and especially of other functions is perhaps most clearly illustrated by the following remark. While we can speak of the logarithm of the sine of a number and of the logarithm of the pressure of a gas, we cannot speak of the pressure of the logarithm of a number nor of the pressure of the temperature of a gas.

Is w A FUNCTION OF u? If γ_1 and γ_2 are two acts of manometer reading belonging to the class Γ such that $p\gamma_1 = p\gamma_2$, and if γ_1' and γ_2' are two acts of volumetric reading (belonging to Γ') simultaneously performed on the same gas samples as γ_1 and γ_2, respectively, then it is found that $v\gamma_1' = v\gamma_2'$. Consequently, the class of all pairs of numbers $(p\gamma, v\gamma')$ for any element γ of Γ and the corresponding element γ' of Γ', is a function—a fact expressed by saying that the v.q. v is a function of the v.q. p, where the pairing of simultaneous acts on the same sample is tacitly understood.[1] Inasmuch as the v.q.'s p and v have been assumed to be invariant with respect to parallel observations (simultaneously performed on the same sample by different observers), the pair (γ, γ') may be replaced by any pair (γ_1, γ_1') where γ_1 is equivalent to γ, and γ_1' is equivalent to γ'. Similarly, the class of all pairs $(r\kappa, a\kappa)$ for any circle κ is a function—a fact expressed by saying that a is a function of r, the pairing of each κ with itself being tacitly understood.

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[1] A v.q., in particular, a function may be a class consisting of only one pair. It would be difficult to exclude such trivial examples. On the other hand, their inclusion does not seem to impair the significance of the function concept.

[2] Here, to be quite precise, instead of 'functions' we should say 'v.q.'s the domains of which contain numbers'.

[3] For every element of dom I' $(=$ dom $f)$, the function I' assumes the same value as I. But dom I' may be a proper part of dom I, in which case I is called an *extension* of I, and I' a *restriction* of I.

Let u and w be any two v.q.'s, say, the classes of all pairs $(\alpha, u\alpha)$ for any α belonging to A, and $(\beta, w\beta)$ for any β belonging to B. Is w a function of u? No attempt will here be made to answer this question absolutely. It will, however, be answered relative to each one-to-one mapping of a part of A on a part of B, that is, to a class Π of pairs (α, β) such that each element of A (resp. B) is the first (resp. second) member of at most one pair belonging to Π. We say that *w is a function of u relative to Π*, provided that, if (α_1, β_1) and (α_2, β_2) belong to Π and if $u\alpha_1 = u\alpha_2$, then also $w\beta_1 = w\beta_2$. In this case, the class of all pairs $(u\alpha, w\beta)$ for any pair (α, β) belonging to Π is a function.

It may be that w is a function of u relative to some mappings but not to others. Suppose two oscillators of equal period have a phase difference of 1/4 of a period. Relative to pairs of simultaneous observations of the positions of the oscillators, neither position is a function of the other. Relative to pairs of observations differing in time by 1/4 of a period, either position is a function of the other. Relative to certain other pairs, one position is a function of the other but not vice versa.

WHAT FUNCTION OF u IS w? Boyle not only recognised the class of all pairs $(p\gamma. v\gamma')$ as a function, but, if proper units are chosen, identified it with a simple known function, I^{-1}. For he found that $v\ ' = 1/p\gamma$ for each γ belonging to Γ; hence his function is the class of all[2] pairs $(p\gamma, 1/p\gamma)$. For falling objects, Galileo implicitly introduced the class of pairs (τ, σ) each consisting (1) of an act τ of reading a timer, set in motion when an object is dropped, and (2) a *simultaneous* act σ of observing the scale mark opposite *the same* object. If $t\tau$ and $s\sigma$ are the numerical results in seconds and feet, then $s\sigma = 16(t\tau)^2$; that is to say, the class of pairs $(t\tau, s\sigma) = (t\tau, 16(t\tau)^2)$ is identical with the function $\underline{\underline{16}}.I^2$.[1] (Here, in the sense of logic, τ is a variable with the range T).

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[1] cf. N. R. Campbell, *Physics. The Elements*, Cambridge, 1920, p. 329 ff.

[2] The domain of Boyle's function, being the range of the variable quantity p, consists of positive numbers only; the function really is a restriction of I^{-1} in the sense of footnote [3] on p. 137.

Traditionally, physical laws are expressed in formulae such as $v = 1/p$, $s = 16t^2$ or, more generally, $w = fu$. No reference is made to elements of the domains, and the formulae contain no variables in the sense of logic. A law in this form equates a v.q. with what may be called a *denominate function*, just as a statement about a mensuration equates some property of an object and a denominate number ; if R is the length of some rod, $R = 7\frac{1}{2}$ ft. and generally $B = cA$ for two like properties of objects. Such results can be formulated in terms of pure numbers : $(R$ in ft.$) = 7\frac{1}{2}$ and $(B$ in $A) = c$. Similarly, if v_p, s_t, and w_u denote the functions connecting v with p, s with t, and w with u, the physical laws can be formulated in terms of pure functions : $v_p = I^{-1}$, $s_t = \underline{\underline{16}}.I^2$, and $w_u = f$. The variable quantities, p, t, u (corresponding to the units, ft. and A) are called *independent*, v, s, w, *dependent*. Incidentally, there is a more far-reaching analogy between the functional connection of v.q.'s and the numerical connection of properties, with substitution of functions replacing multiplication of numbers. For instance, relative to proper mappings of the domains : $w_v = g$, $v_u = h$, and $w_u = f$ imply $f = gh$, just as $(C$ in $B) = d$, $(B$ in $A) = e$, and $(C$ in $A) = c$ imply $c = d \cdot e$; and $v_v = I$, just as $(B$ in $B) = 1$.

Traditionally, the functions v_p, s_t, and w_u are denoted by $v(p)$, $s(t)$, and $w(u)$. A comparison of the last three symbols with the symbols '$\log x$' and '$f(x)$' strongly suggests (1) that v, s, and w are functions such as log and f; (2) that x is of the same nature as p, t, and u. But neither is the case : (1) v and s are scientific v.q.'s (certainly not functions) and w is any v.q. ; (2) x is a numerical variable and not a v.q. such as p, t, and u. In the formula $D \log x = 1/x$, the numerical variable, the range of which is the class of all positive numbers, can be replaced (a) by any number (say 3) belonging to the range : $D \log 3 = 1/3$; (b) by any other numerical variable (say y)

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[1] More precisely, instead of 'identical with' we should say 'a restriction of' (in the sense of footnote [3] on p. 137).

having the same range: $D \log y = 1/y$. The nature of t in $s(t)$ is *toto caelo* different: (*a*) t in $s(t)$ cannot be replaced by 3 since the symbol $s(3)$ does not reveal which distance is meant: that when the time is 3 or that when the velocity c is 3 or that when some other scientific v.q. is 3. Actually, physicists avoid the symbol $s(3)$ and write $s(t = 3)$, $s(c = 3)$, or whatever the situation may demand. (*b*) t in $s(t)$ cannot be replaced by c or any letter symbolising any v.q. other than the time.

VARIABLE QUANTITIES AND FUNCTIONS IN PURE AND APPLIED ANALYSIS. In analysis, derivatives and integrals of functions are defined by well-known limiting processes. In theorems such as $D \sin x = \cos x$, the numerical variable x can be completely dropped. Only the lack of a traditional symbol for the power functions has heretofore made it impossible to write $D \log = I^{-1}$ and $D(\underline{\underline{16}}.I^2) = \underline{\underline{32}}.I$, just as $D \sin = \cos$. In the formula $\int_a^b \cos x \, dx = \sin b - \sin a$, the '$x \, dx$' adds nothing to the meaning of the integral which therefore can, without ambiguity, be written $\int_a^b \cos$. Similarly, $\int_1^3 I^{-1} = \log 3$. In fact, the elimination of the numerical variables results in a remarkable unification, algebraisation, and streamlining of the formulae of calculus.[1]

But how can results about $\int_a^b \cos$ or $\int_c^d f$ (which contain no numerical variables) be applied to a force p, considering that it is necessary to distinguish between impulse (the integral of p with regard to the time t) and work (the integral of p with regard to the distance s)? The answer is as follows: Since integration is defined for functions, and p is a scientific v.q. and not a function, $\int_a^b p$ is a meaningless symbol; but the functions connecting p with other v.q.'s such as t and s can be integrated: $\int_a^b p_t$ (traditionally written $\int_a^b p \, dt$) is an impulse, whereas $\int_c^d p_s$ (or $\int_c^d p \, ds$) is work. For an elastic force, connected with the time by $p_t = \cos$, the impulse is $\int_a^b p_t = \int_a^b \cos = \sin b - \sin a$. If p is connected with s by a Hooke function $p_s = -I$, then the work is $\int_c^d p_s = \int_c^d (-I) = \frac{1}{2}(c^2 - d^2)$. Similarly, if v is the volume of an expanding sphere (which is a scientific v.q. and not a function), 'Dv' is meaningless, but the function v_t connecting v with the time may have a derivative Dv_t; and certainly, if r is the radius, $Dv_r = D(\underline{\underline{\tfrac{4}{3}\pi}}.I^3) = \underline{\underline{4}}\pi \cdot I^2$. To ask for the square root or the log of a city is senseless. But it is possible to compute the square root of the number connecting the city with a square foot (its area in square feet), and the log of the number connecting the city to an inhabitant (its population).

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[1] cf. the Introduction and Chapters VIII and IX of *Calculus: A Modern Approach*, quoted in footnote [2] on p. 134

In Dv_r and $\int_a^b p_t$ the (independent) scientific v.q.'s r and t are just as important as v and p. But in $D \sin x = \cos x$ and $\int_0^{\pi/2} \cos x \, dx = 1$, the numerical variable x is not as important as the function sin and cos. In fact, in these formulae x is completely superfluous. The tacit identification of independent v.q.'s in denominate functions and numerical variables in the formulae of calculus is a century-old misunderstanding which can be clearly traced to the three principal shortcomings of traditional analysis: (1) the equivocal use of the term 'variable', sometimes in the sense of Weierstrass, sometimes in the sense of a scientific v.q., and sometimes in the sense of a general v.q.; (2) the unfortunate notation which suggests a parallelism between $p(t)$ and $f(x)$, and conceals the true analogies between $p(t)$ and f, as well as between $p(t = 3)$ and $f(3)$; (3) the lack of a symbol for the nth power function, which has necessitated the use of numerical variables in formulae such as $D(16\,x^2) = 32\,x$.

Moreover, little attention has been paid to the fact that differential calculus deals with (1) a *pure* operator, D (a symbol introduced by Arbogast, whereas Lagrange used a dash, and Heaviside the letter 'p'); (2) an operator here called *denominate* (which Leibniz denoted by 'd/dx', Cauchy by 'D_x'). Newton's dot, inasmuch as it is used for differentiation with respect to the time, is implicitly denominate. The pure operator associates a function with a function, whereas the denominate operator associates a v.q. with a pair of v.q.'s. For instance, ds/dt $(= \dot{s})$ and dv/dp are scientific v.q.'s (the velocity and the rate of expansion with respect to pressure) and so are the impulse from a on, and the work from c on, which may be denoted by $\int_a p\,dt$

and $\int_c p\, ds$. In general, dw/du and $\int_a w\, du$ denote v.q.'s (the rate of change w with u, and the cumulation of w with u). These are connected with u by the functions $(dw/du)_u = Dw_u$ and $(\int_a w\, du)_u = \int_a w_u$.

From the theorem of pure calculus (1) $D \sin = \cos$ it follows that $w_u = \sin$ implies $Dw_u = \cos$ or, in denominate form : (2) If u and w are v.q.'s such that $w = \sin u$, then $dw/du = \cos u$. Without reference to w, (2) assumes the form : (3) If u is any v.q., such as time or pressure, then $d \sin u/du = \cos u$. (N.B. (3) must not be confused with the statement : If x is any number, such as 1 or $\pi/2$, then $D \sin x = \cos x$.) Formulation (1) follows the usage of the physicist who writes $v = 1/p$ rather than $v\gamma' = 1/p\gamma$. Expressing Theorem (1) in the forms (2) and (3) is comparable to expressing the arithmetical result (1′) $15 = 3 \cdot 5$ in the forms (2′) If A and B are two objects such that $B = 15A$, then $B = 3 \cdot 5A$ or (3′) If A is any object, such as a penny or an apple, then $15A = 3 \cdot 5A$. Arithmeticians, except when instructing very young children, confine their statements to the realm of pure numbers without in any way jeopardising the applicability of their results. They never stoop to references to objects (not even to objects of the most general kind). It seems likely that, once the notion of 'variable' is fully elucidated, analysts will rise above the level of references to v.q.'s (even to v.q.'s of the most general kind) without in any way jeopardising the applicability of their results. They will confine their statements to the realm of pure functions, such as sine, $\underline{\underline{16}}.I^2$, and f. The introduction of a symbol for the identity function makes it possible to dispense with numerical variables and to express theorems in formulae such as $D \log = I^{-1}$, $D(f^2) = \underline{\underline{2}} \cdot f \cdot Df$, $\int_0^1 \exp. = e - 1$. These formulae can be directly applied to functions connecting variable quantities.[1]

Illinois Institute of Technology
Chicago

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[1] In writing this paper the author has greatly profited from suggestions of Dr James K. Senior.

NOTES AND COMMENTS

Degree of Confirmation

1. The purpose of this note is to propose and to discuss a definition, in terms of probabilities, of *the degree to which a statement x is confirmed by a statement y.* I shall take this to be identical with *the degree to which a statement y confirms a statement x.* I shall denote this degree by the symbol ' $C(x, y)$ ', to be pronounced ' the *degree of confirmation of x by y* '. In particular cases, x may be a hypothesis, h; and y may be some empirical evidence, e, in favour of h, or against h, or neutral with respect to h. But $C(x, y)$ will be applicable to less typical cases also.

The definition is to be in terms of probabilities. I shall make use of both, $P(x, y)$, i.e. the (relative) probability of x given y, and $P(x)$, i.e. the (absolute) probability of x.[1] But any one of these two would be sufficient.

2. It is often assumed that the degree of confirmation of x by y must be the same as the (relative) probability of x given y, i.e. that $C(x, y) = P(x, y)$. My first task is to show the inadequacy of this view.

3. Consider two contingent statements, x and y. From the point of view of the confirmation of x by y, there will be two extreme cases: the complete support of x by y or the establishment of x by y, when x follows from y; and the complete undermining or refutation or disestablishment of x by y, when $\bar{x}$ follows from y. A third case of special importance is that of mutual independence or irrelevance, characterised by $P(xy) = P(x)P(y)$. Its value of $C(x, y)$ will lie below establishment and above disestablishment.

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[1] ' $P(x)$ ' may be defined, in terms of relative probability, by the definiens '$P(x, \overline{z\bar{z}})$' or, more simply, ' $P(x, \overline{x\bar{x}})$ '. (I use throughout ' xy ' to denote the conjunction of x and y, and ' $\bar{x}$ ' to denote the negation of x.) Since we have, generally, $P(x, y\overline{z\bar{z}}) = P(x, y)$, and $P(x, yz) = P(xy, z)/P(y, z)$, we obtain $P(x, y) = P(xy)/P(y)$—a serviceable formula for defining relative probability in terms of absolute probability. (See my note in *Mind*, 1938, **47**, 275, f., where I identified absolute probability with what I called ' logical probability ' in my *Logik der Forschung*, Vienna, 1935, esp. sects. 34 f. and 83, since the term ' logical probability ' is better used for the ' logical interpretation ' of both $P(x)$ and $P(x, y)$, as opposed to their ' statistical interpretation ' which may be ignored here.)

Between these three special cases—establishment, independence, and disestablishment—there will be intermediate cases : *partial support* (when y entails part of the content of x) ; for example, if our contingent y follows from x but not vice versa, then it is itself part of the content of x and thus entails part of the content of x, supporting x ; and *partial undermining* of x by y (when y partially supports x) ; for example, if y follows from $\bar{x}$. We shall say, then, that y supports $\bar{x}$, or undermines x, whenever $P(xy)$ or $P(\bar{x}y)$, respectively, exceed their values for independence. (The three cases—support, undermining, independence—are easily seen to be exhaustive and exclusive on this definition.)

4. Consider now the conjecture that there are three statements, x_1, x_2, and y, such that (i) x_1 and x_2 are each independent of y (or undermined by y) while (ii) y supports their conjunction x_1x_2. Obviously, we should have to say in such a case that y confirms x_1x_2 to a higher degree than it confirms either x_1 or x_2 ; in symbols,

$$C(x_1, y) < C(x_1x_2, y) > C(x_2, y) \qquad (4.1)$$

But this would be incompatible with the view that $C(x, y)$ is a probability, i.e. with

$$C(x, y) = P(x, y) \qquad (4.2)$$

since for probabilities we have the generally valid formula

$$P(x_1, y) \geqslant P(x_1x_2, y) \leqslant P(x_2, y) \qquad (4.3)$$

which, in the presence of (4.1) contradicts (4.2). Thus we should have to drop (4.2). But in view of $0 \leqslant P(x, y) \leqslant 1$, (4.3) is an immediate consequence of the general multiplication principle for probabilities. Thus we should have to discard such a principle for the degree of confirmation. Moreover, it appears that we should have to drop the special addition principle also. For a consequence of this principle is, since $P(x, y) \geqslant 0$,

$$P(x_1x_2 \text{ or } x_1\bar{x}_2, y) \geqslant P(x_1x_2, y) \qquad (4.4)$$

But for $C(x, y)$, this could not remain valid, considering that the alternative, x_1x_2 *or* $x_1\bar{x}_2$, is equivalent to x_1, so that we obtain by substitution on the left-hand side of (4.1) :

$$C(x_1x_2 \text{ or } x_1\bar{x}_2, y) < C(x_1x_2, y) \qquad (4.5)$$

In the presence of (4.4), (4.5) contradicts (4.2).[1]

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[1] In his *Logical Foundations of Probability*, Chigago, 1950, p. 285, Carnap uses the multiplication and addition principles as '*conventions on adequacy*' *for the degree of confirmation*. The only argument he offers in favour of the adequacy of these

5. These results depend upon the conjecture that statements x_1, x_2, and y exist such that (i) x_1 and x_2 are each independent of y (or undermined by y) while (ii) y supports x_1x_2. I shall prove this conjecture by an example.[1]

Take coloured counters, called ' *a* ', ' *b* ', . . . , with four exclusive and equally probable properties, blue, green, red, and yellow. Let x_1 be the statement ' *a* is blue or green ' ; $x_2 =$ ' *a* is blue or red ' ; $y =$ ' *a* is blue or yellow '. Then all our conditions are satisfied. (That y supports x_1x_2 is obvious : y follows from x_1x_2, and its presence raises the probability of x_1x_2 to twice the value it has in the absence of y.)

6. But we may even construct a more striking example to show the inadequacy of identifying $C(x, y)$ and $P(x, y)$. We choose x_1 so that it is strongly supported by y, and x_2 so that it is strongly undermined by y. Thus we shall have to demand that $C(x_1, y) > C(x_2, y)$. But x_1 and x_2 can be so chosen that $P(x_1, y) < P(x_2, y)$. The example is this : take $x_1 =$ ' *a* is blue ' ; $x_2 =$ ' *a* is not red ' ; and $y =$ ' *a* is not yellow '. Then $P(x_1) = \frac{1}{4}$; $P(x_2) = \frac{3}{4}$; and $\frac{1}{3} = P(x_1, y) < P(x_2, y) = \frac{2}{3}$. That y supports x_1 and undermines x_2 is clear from these figures, and also from the fact that y follows from x_1 and also from $\bar{x}_2$.

7. Why have $C(x, y)$ and $P(x, y)$ been confounded so persistently ? Why has it been ignored that it is absurd to say that some evidence y of which x is completely independent can yet strongly ' confirm ' x ? And that y can strongly ' confirm ' x, even if y undermines x ? And this even if y is the total available evidence ? I do not know the answer to this question, but some suggestions may be helpful. There is first the powerful tendency to think that whatever may be called the ' likelihood ' or ' probability ' of a hypothesis must be a probability in the sense of the calculus of probabilities. In order to disentangle the various issues here involved, I distinguished twenty years ago what I then called the ' degree of confirmation ' from both, the logical and the

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principles is that ' they are generally accepted in practically all modern theories of probability$_1$', i.e. our $P(x, y)$ which Carnap identifies with the ' degree of confirmation '. But the very term ' degree of confirmation ' (' *Grad der Bewährung* ') was introduced by me in sections 82 f. of my *Logik der Forschsung* (a book to which Carnap sometimes refers), in order to show that both logical and statistical probability are *inadequate* to serve for a degree of confirmation, since confirmability must increase with testability, and thus with (absolute) logical improbability and content. (See below.)

[1] The example satisfies (i) for *independence* rather than *undermining*. (To obtain one for undermining, add amber as a fifth colour, and put $y =$ ' *a* is amber or blue or yellow '.)

statistical probability. But unfortunately, the term ‘ degree of confirmation ’ was soon used by others as a new name for (logical) probability; perhaps under the influence of the mistaken view that science, unable to attain certainty, must aim at a kind of ‘ *Ersatz* ’—at the highest attainable probability.

Another suggestion is this. It seems that the phrase ‘ the degree of confirmation of x by y ’ was never turned round into ‘ the degree to which y confirms x ’, or ‘ *the power of y to support x* ’; for in this form it would have been quite obvious that, in a case in which y supports x_1 and undermines x_2, $C(x_1, y) > C(x_2, y)$ is absurd—although $P(x_1, y) > P(x_2, y)$ may be quite in order, indicating, in such a case, that we had $P(x_1) > P(x_2)$ to start with. Furthermore, there seems to be a tendency to confuse measures *of* increase or decrease with the measures *that* increase and decrease (as shown by the history of the concepts of velocity, acceleration, and force). But the power of y to support x, it will be seen, is essentially a *measure of the increase or decrease* due to y, in the probability of x. (See also 9 (vii), below.)

8. It will perhaps be said, in reply to all this, that it is in any case legitimate to call $P(x, y)$ by any name, and also by the name ‘ degree of confirmation ’. But the question before us is not a verbal one.

The degree of confirmation of a hypothesis x by empirical evidence y is supposed to be used for estimating the degree to which x is *backed by experience*. But $P(x, y)$ cannot serve this purpose, since $P(x_1, y)$ may be higher than $P(x_2, y)$ even though x_1 is undermined by y and x_2 supported by y, and since this is due to the fact that $P(x, y)$ depends very strongly upon $P(x)$, i.e. the absolute probability of x, which has nothing whatever to do with the empirical evidence.

Furthermore, the degree of confirmation is supposed to have an influence upon the question whether we should *accept*, or *choose*, a certain hypothesis x, if only tentatively; a high degree of confirmation is supposed to characterise a hypothesis as ‘ good ’ (or ‘ acceptable ’), while a disconfirmed hypothesis is supposed to be ‘ bad ’. But $P(x, y)$ cannot help here. *Science does not aim, primarily, at high probabilities. It aims at a high informative content, well backed by experience. But a hypothesis may be very probable simply because it tells us nothing, or very little.* A high degree of probability is therefore not an indication of ‘ goodness ’—it may be merely symptom of low informative content. On the other hand, $C(x, y)$ must, and can, be so defined that only hypotheses with a high informative content can reach high degrees of confirmation. The *confirmability* of x (i.e. the maximum degree of confirmation which a statement x can reach) should increase with $C(x)$, i.e. the measure of the content of x, which is equal to $P(\bar{x})$, and therefore to the *degree of testability* of x. Thus, while $P(\overline{x\bar{x}}, y) = 1$, $C(\overline{x\bar{x}}, y)$ should be zero.

9. A definition of $C(x, y)$ that satisfies all these and other *desiderata* indicated in my *Logik der Forschung*, and stronger ones besides, may be based upon $E(x, y)$, i.e. a non-additive measure of the *explanatory power of* x *with respect to* y, designed so as to have -1 and $+1$ as its lower and upper bounds. It is defined as follows.

Let x be consistent,[1] and $P(y) \neq 0$; then we define,

$$E(x, y) = \frac{P(y, x) - P(y)}{P(y, x) + P(y)} \tag{9.1}$$

$E(x, y)$ may also be interpreted as a non-additive measure of the dependence of y upon x, or the support given to y by x (and vice versa). It satisfies the most important of our *desiderata*, but not all: for example, it violates (viii, *c*) below, and satisfies (iii) and (iv) only approximately in special cases. To remedy these defects, I propose to define $C(x, y)$ as follows.

Let x be consistent and $P(y) \neq 0$; then we define,

$$C(x, y) = E(x, y)(1 + P(x)P(x, y)) \tag{9.2}$$

This is less simple than, for example, $E(x, y)(1 + P(xy))$, which satisfies most of our *desiderata* but violates (iv), while for $C(x, y)$ the theorem holds that it satisfies all of the following *desiderata*:

(i) $C(x, y) \gtreqless 0$ respectively if and only if y supports x, or is independent of x, or undermines x.

(ii) $-1 = C(\bar{y}, y) \leq C(x, y) \leq C(x, x) \leq 1$

(iii) $0 \leq C(x, x) = C(x) = P(\bar{x}) \leq 1$

Note that $C(x)$, and therefore $C(x, x)$, is an additive measure of the content of x, definable by $P(\bar{x})$, i.e. the absolute probability of x to be false, or the *a priori* likelihood of x to be *refuted*. Thus *confirmability* equals *refutability or testability*.[2]

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[1] This condition may be dropped if we accept the general convention that $P(x, y) = 1$ whenever y is inconsistent.

[2] See section 83 of my *L.d.F.*, which bears the title 'Confirmability, Testability, Logical Probability'. (Before 'logical', 'absolute' should be inserted, in agreement with the terminology of my note in *Mind*, loc. cit.)

(iv) If y entails x, then $C(x, y) = C(x, x) = C(x)$

(v) If y entails $\bar{x}$, then $C(x, y) = C(\bar{y}, y) = -1$

(vi) Let x have a high content—so that $C(x, y)$ approaches $E(x, y)$—and let y support x. (We may, for example, take y to be the total available empirical evidence.) *Then for any given* y, $C(x, y)$ *increases with the power of* x *to explain* y (i.e. to explain more and more of the content of y), and therefore with the *scientific interest* of x.

(vii) If $C(x) = C(y)$ then $C(x, u) \gtreqless C(y, w)$ whenever $P(x, u) \gtreqless P(y, w)$.

(viii) If x entails y, then : (*a*) $C(x, y) \geqslant 0$; (*b*) for any given x, $C(x, y)$ and $C(y)$ increase together ; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.[1]

(ix) If $\bar{x}$ is consistent and entails y, then : (*a*) $C(x, y) \leqslant 0$; (*b*) for any given x, $C(x, y)$ and $P(y)$ increase together ; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.

10. All our considerations, without exception, may be relativised with respect to some initial information z ; adding at the appropriate places phrases like 'in the presence of z, assuming $P(z, \overline{z\bar{z}}) \neq 0$'. The relativised definition of the degree of confirmation becomes :

$$C(x, y, z) = E(x, y, z)(1 + P(x, z)P(x, yz)) \tag{10.1}$$

where

$$E(x, y, z) = \frac{P(y, xz) - P(y, z)}{P(y, xz) + P(y, z)} \tag{10.2}$$

$E(x, y, z)$ is the explanatory power of x with respect to y, in the presence of z.[2]

11. There are, I believe, some intuitive *desiderata* which cannot be satisfied by any formal definition. For example, a theory is the better confirmed the more ingenious our unsuccessful attempts at its refutation have been. My definition incorporates something of this idea—

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[1] (vii) and (viii) contain the only important desiderata which are satisfied by $P(x, y)$.

[2] Let x_1 be Einstein's gravitational theory ; x_2 Newton's ; y the (interpreted) empirical evidence available today, including 'accepted' laws (it does not matter if none or one or both of the theories in question is included, provided our conditions for y are satisfied) ; and z a part of y, for example, a selection from the evidence available one year ago. Since we may assume that x_1 explains more of y than x_2, we obtain $C(x_1, y, z) \geqslant C(x_2, y, z)$ for every z, and $C(x_1, y, z) > C(x_2, y, z)$ for any suitable z containing some of the relevant initial conditions. This follows from (vi)—even if we have to assume that $P(x_1, yz) = P(x_2, yz) = P(x_1) = P(x_2) = 0$.

if not as much as can be formalised. But one cannot completely formalise the idea[1] of a sincere and ingenious attempt.

The particular way in which $C(x, y)$ is here defined I consider unimportant. What may be important are the *desiderata*, and the fact they can be satisfied together.

KARL R. POPPER

The University of London

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[1] There are many ways of getting nearer to this idea. For example, we may put a premium on crucial experiments by defining

$$C_{a,\,b}(h) = \left(C(h, e_b)\prod_{i=1}^{n} C(h, c_i, e_a)\right)^{1/(n+1)}$$

where $c_1, c_2, \ldots,$ is the sequence of experiments made between the moments of time, t_a and t_b. We have $t_a < t_1 \leqslant t_i \leqslant t_n = t_b$. e_a and e_b are the total evidence (which may include laws) *accepted* at t_a and t_b. We postulate $P(c_i, e_b) = 1$ and (to ensure that only new experiments are counted) $P(c_i, e_a) \neq 1$ and $P(c_i, Uc_j) \neq 1$. (‘Uc_j’ *is the spatio-temporal universalisation of* c_j.)

A Note on a Suggested Modification of Newton's Corpuscular Theory of Light to Reconcile it with Foucault's Experiment of 1850

IT WILL be remembered that Newton's theory assumed that, when a ray of light passes into a denser medium, the perpendicular component of its incident velocity is accelerated owing to an attractive force acting perpendicularly to the refracting surface, and consequently the component parallel to the surface remains constant. This gives the result, in contradiction with both the wave theory and Foucault's experiment of 1850 (proving that the velocity of light in water is less than in air) that the actual velocity will be greater in the medium of refraction.

The view was expressed by the late Alexander Wood—in the course of a discussion of the rôle of crucial experiments in physics[1] —that in order to reconcile Newton's theory, it would have been sufficient to introduce the following assumption as a way out: namely, that when the light goes into a denser medium *the perpendicular component of the incident velocity remains unchanged*, while the parallel component is diminished by action of a frictional kind. This assumption would yield the consequence that the light will travel more slowly after refraction than before ; and the theory would thus be brought into agreement with the experiment, which would then cease to be crucial.

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[1] Alexander Wood, *In Pursuit of Truth*, London, 1927, 47-48

Since the appearance of Wood's book his suggestion has been endorsed by Florian Cajori in the Appendix which he wrote to his edition of Newton's *Principia*.[1]

It should be first pointed out that Wood's particular suggestion, if quantitively expressed, can be easily seen to lead to at least one result (viz. a *false* law of refraction) which would be enough to dismiss it at once.[2]

Concerning the general question whether any such *ad hoc* amendment is at all possible I should like to make the following comment. The corpuscular theory that was confronted with Foucault's experiment is not contained in the hypothesis ' light consists of particles or discrete quantities ', but rather in the hypothesis ' rays of light follow the laws of (Newtonian) particle dynamics '. Obviously the first hypothesis was not refuted by the experiment, since that proposition—by itself and without further specifications—cannot be related to any kind of test. The second hypothesis, however, *was* refuted —once and for all. For if one looks at refraction from the standpoint of the classical theory of dynamics one is obliged to start from a principle which is verified in that theory, namely, Maupertuis' principle of least action. This principle necessarily leads to the result which was found false by Foucault's experiment. No suggestion therefore of the kind proposed by Wood would have been of any help, or indeed available. If, on the other hand, we look at refraction from the point of view of the wave theory we have to start from Fermat's principle of least time which follows in its turn from wave considerations. Fermat's principle leads to the consequence that the velocity of light is inversely proportional to the density of the medium

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[1] Newton, *Principia*, California, 1947, Appendix, note 26, 651-52

[2] For his proposed assumption concerning the perpendicular component can be expressed thus :

$$v_i \cos i = v_r \cos r,$$

where i, r are the angles of incidence and of refraction ; and v_i, v_r, the velocities of incidence and of refraction respectively.

Combining this with the definition of the refractive index n,

$$v_r = n\, v_i,$$

a cosine law of refraction is obtained :

$$\cos i = n \cos r.$$

it is traversing. De Broglie has indeed reconciled these two mutually contradictory principles ; but this was achieved within the framework of a new mechanics making use of the relativistic relation $E = mc^2$ and Planck's quantum of action, that is two concepts completely foreign to Newtonian mechanics. The view usually expressed nowadays, arguing from the existence of wave mechanics to the conclusion that Foucault's experiment did not decide against Newton's theory, is thus misleading in two respects—first with regard to what was at issue when the experiment was performed ; and second in understanding what has happened since, which was more than an artificial modification to accommodate an experimental result.

A. I. SABRA

E. A. Milne's Scales of Time

ALTHOUGH the three-fold assertion made by Dr Adolf Grünbaum at the end of his note[1] is perfectly satisfactory, I cannot help thinking that he may have given some readers a false impression of Milne's point of view. I hold no brief for T. Page, but I can say with some confidence that Milne would have regarded as a distortion of his theory the argument that the *mathematical* fact that $\tau = -\infty$ corresponds to $t = 0$ in the transformation

$$\tau = t_0 \log (t/t_0) + t_0$$

warrants the *physical* conclusion ' that $t = 0$ marks an absolute beginning of creation '. Milne always regarded the t-scale as fundamental, and believed, rightly or wrongly, that the singular epoch $t = 0$ marked an absolute beginning of the universe because it was associated with a singular state (point-origin) of his uniformly expanding world-model. This belief was based on physical rather than on mathematical grounds. The result that the singular epoch could be transformed into $\tau = -\infty$ he deduced as a mathematical ' corollary ' of this physical ' theorem ', and not the other way round as Dr Grünbaum appears to suggest.

G. J. WHITROW

Imperial College of Science and Technology
London, S.W. 7

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[1] A. Grünbaum, this *Journal*, 1954, 4, 329

Note on a Passage in Berkeley's De Motu

In § 43 of *De Motu* Berkeley quotes the view of 'a well known recent writer' that 'nihil in motu esse reale praeter momentaneum illud quod in vi ad mutationem nitente constitui debet'.[1] In a footnote Dr Luce states that editors attribute this statement to Newton, without giving any reference, but is himself doubtful about it. The sentence (which actually runs 'Nihilque adeo in ipso reale est . . .') is from Leibniz's *Specimen Dynamicum, Acta Eruditorum* 1695, from which Berkeley had already quoted in § 8 and § 20.[2] Leibniz argues that nothing really exists unless all its parts coexist. This clearly is not the case with motion. Leibniz goes on: 'Thus nothing is real in it save that momentary thing which must be ascribed to (literally, "set" or "placed in") force striving toward change.' Dr Luce's translation is 'which must be constituted when a force is striving toward a change', which does not quite bring out what Leibniz is saying.

L. J. Russell

[1] *Works*, ed. by A. A. Luce and T. E. Jessop, **4**, 22

[2] Gerhard, *Math. Works of Leibniz*, **6**, 235

REVIEWS

THE MIND-BRAIN PROBLEM

As suggested by its title, Professor Eccles's book[1] in effect deals with two subjects, namely, the principles of neurophysiology and the relationship between mind and brain. The first and larger part of the book—seven out of eight chapters—is devoted to the principles of neurophysiology. It begins with the study of the physical and chemical properties of the membrane and of the axoplasm of nerve fibres, as a basis for explaining the initiation and propagation of impulses in the fibres. The transmission of excitation from cell to cell at the neuromuscular junction and at the synapses of the sympathetic ganglion is then studied. This leads to the investigation of synaptic transmission in the central nervous system and to a discussion of the mechanism of spinal reflexes including the phenomena of inhibition and facilitation. The prolonged functional changes (' plasticity ') which can occur in the nervous system are studied, suggesting an explanation of the act of learning and of conditioned reflexes. The seventh chapter gives a masterly exposition of the main aspects of the electrophysiology of the cerebral cortex. These neurophysiological chapters are packed with quantitative information, including very recent developments in the field, a number of which originate from the author's own laboratory. The author does not cover the entire field of neurophysiology, but in a book of the present kind a certain amount of selection is inevitable. Even though the theories relating to the functioning of the central nervous system, and particularly of the cortex, are bound to be less safely established than those relating to isolated elements such as nerve fibres, the book gives an admirably critical and concrete exposition of the neurophysiological principles. While by the nature of things many of the subjects discussed, such as the physico-chemical and biological properties of the nerve membrane, are difficult, the reader is helped by the systematic way in which the matter is presented. This work therefore may be warmly recommended to those interested in the latest advances in neurophysiology.

The first part of the book may be considered as a preparation for the eighth and last chapter, which is concerned with the mind-brain problem. This chapter, which may with advantage be read in conjunction with the Preface, raises important issues which should be of particular interest to readers of this *Journal.* The author puts forward a clear-cut theory of the interaction of the mind with the body, the crux of the theory being that the active cerebral cortex would possess 'unique detector properties', such that minute 'mind influences' would be able to act upon some of the synapses of the cortex and displace material constituents of these synapses in certain definite ways. Thus the neural network would be endowed with very special properties which would make the action of mind upon matter possible. Once the possibility of such action is accepted, the author shows how the minute material changes which it would induce in only a few cells of the cortex would be able to affect the behaviour of the organism in a readily observable manner. 'The will' writes the author 'modifies the spatio-temporal activity of the neural network by exerting spatio-temporal "fields of influence" that become effective through this unique detector function of the active cerebral cortex. It will be noted that the "will" or "mind influence" has itself a spatio-temporal patterned character in order to allow it this operative effectiveness.' As far as I know, Professor Eccles's theory is the most precise statement, in up-to-date scientific terms, of the kind of interactionist doctrine whose origin is generally attributed to Descartes. It is perhaps the most explicit theory concerning the mechanism of interaction between mind and brain which has been put forward since the time of Descartes himself. This chapter should therefore enable one to examine the interactionist doctrine in greater detail than has been possible hitherto. Some comments will be presented here on one or two points.

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[1] *The Neurophysiological Basis of Mind: The Principles of Neurophysiology:* being the Waynflete Lectures delivered in the College of St. Mary Magdalen, Oxford, in Hilary Term 1952, J. C. Eccles. (The Clarendon Press, Oxford, 1953. Pp. viii + 314. 25s.)

The author writes in the Preface: 'Even in the more speculative chapter the method is essentially scientific, since attempts are there made to formulate hypotheses which lead to predictions that in part at least are testable.' This does raise the important question whether the author's theory of interaction can indeed be tested by experiment. What the author means by testable 'in part' is not quite clear, but undoubtedly the real point at issue is whether scientific experiments could establish, or refute, the author's theory. In particular, we may inquire whether it would be possible to differentiate between the two following hypotheses: (*a*) the author's own theory, summarised above, concerning the mode of action of the 'will', and (*b*) the mechanistic hypothesis according to which man is nothing but a piece of neurophysiological machinery whose behaviour depends only upon the kind of material influences discussed in the first part of the book.

It will be noted that if (*b*) is false, (*a*) is not necessarily true, at least in the form given it by the author, because various hypotheses can be put forward on mind-brain interaction. But if (*a*) or any other hypothesis according to which mind acts upon the brain is true, then the mechanistic hypothesis (*b*) must be false. Our first task, therefore, should be to try to prove that (*b*) is false if we wish ultimately to prove that (*a*) is true.

A difficulty arises from a consequence of the author's theory, namely, the existence at some stage in the human brain of two parallel pathways, one neural and the other mental. The author says that the activity of the sensory part of the brain must at some point arouse sensations in the mind. Therefore, we must presume, the mind elaborates these sensations and, as a result of such elaboration, the will is made to act upon the neural network in the manner explained above. Thus we must have a mental pathway, leaving the brain at a place or places which we may call the ' sensory incoming stations ' and returning to the brain at the ' motor outgoing stations '. Now these two ' stations ' or sets of ' stations ' are also connected by the neural network of the brain. The incoming and the outgoing stations will therefore be linked both by a mental pathway and by a neural pathway consisting of parts of the brain. Accordingly the activity of the human organism might be due to the neural pathway functioning alone, to the mental pathway functioning alone or to both pathways functioning together. It is often assumed that when a man is in a state of unconsciousness only the neural pathway is being used. According to hypothesis (*b*) the same would be true in a normal fully-conscious human subject. The neural pathway alone would be used, although it would not necessarily be functioning in the same way as when the subject is unconscious. The behaviour of the organism, including such activities as speech, would not in any way depend on events in the mental pathway, but would depend exclusively upon the pattern of excitation propagated through the neural network of the brain. The latter pattern of course could be influenced by the autonomous activity of the brain and by activity in other parts of the nervous system, whose ' plasticity ' must be borne in mind in the present connection, but it would not be influenced by events in the mental pathway. The whole process would be entirely mechanistic, free will would be an illusion and consciousness an epiphenomenon. Could such a view be disproved by experiments ?

Assume that we have at our disposal the instrument once alluded to by Professor Adrian in a similar context, namely, Sam Weller's ' patent double million magnifyin' gas microscope of hextra power '. This would have enabled Sam Weller to see through a flight of stairs and a deal door, and it would enable us to watch the detailed functioning of the brain through the human skull. The question then is : Could we detect the slight changes which would occur in the nerve cells as a result of the action upon the brain of the ' mind influences ' of the author's theory ? The microscope should make it theoretically possible to determine the position and momentum of the individual molecules, ions, electrons, etc., in the nerve cells, but with certain important limitations. As we all know, these limitations are due to the fact that, according to quantum physics, any measuring instrument reacts upon the elementary particles being measured.

Within the limits set by Heisenberg's uncertainty principle, these reactions caused by the instrument cannot be controlled, nor can their outcome be predicted. The mere fact, therefore, of lighting the brain with some kind of radiation in order to look at it with our microscope will cause a change in the positions and velocities of the particles which we wish to study.

Now the author considers two different suggestions, originally put forward by Eddington, concerning the action of mind upon matter. According to the first suggestion, (i), it seems that the 'mind influences' acting upon the material constituents of the brain would act also *within* the range of uncertainty defined by Heisenberg's relation. Then the mere fact of looking at the living brain would cause an unavoidable disturbance of the same order of magnitude as the effects which we wish to observe. As a result of this, we might be tempted to think that a direct verification of the action upon nerve cells of 'mind influences' working according to suggestion (i) should, for fundamental reasons, be either impossible or extremely difficult. Yet it would be unsafe to draw such a conclusion because the 'mind influences' would not act at random upon the individual particles, so that some result of their action might be detectable in spite of the random disturbance due to the measuring instrument. From a purely theoretical point of view, on the other hand, suggestion (i) seems to lead to inconsistencies, for which reason Eddington rejected it.

According to the second suggestion, (ii), when matter is in liaison with mind there would occur a correlated behaviour of individual particles of matter. This would stand in sharp contrast to the uncorrelated or random behaviour of particles that is postulated in physics. Eddington stated that this correlated behaviour of matter may be 'regarded by us as something "outside physics"'. As far as the present discussion is concerned, it seems that both suggestions would in fact lead either to a breakdown of the randomness of elementary events or to an apparent modification of the value of the probability of events as determined purely by physical theory.

It seems, therefore, that the existence of the effects postulated by the author would mean that what are ordinarily called the 'laws' of physics would *not* be obeyed in the conscious human brain. The ease with which such effects could be detected if they do occur would to a large extent depend on their order of magnitude. As the latter is presumably small, detection should be difficult, but it does not seem that, in principle, it would be impossible. However, there is also a fundamental difficulty of a different kind.

In practice, measuring the behaviour of *all* the individual constituents of the human brain would be so complicated as to be almost impossible. It would be much simpler to observe (taking an example given by the author) one small synaptic knob, about 1 micron in diameter, in order to see whether, all other things being equal, this knob is subjected to systematic displacements relative to the subjacent membrane each time a ' mind influence ' is acting upon it. We are not able, however, to control the influence of the mind as we control a physical agent such as a magnetic or an electric field. We have to rely on inferences from the behaviour of the subject and on deductions derived from what he tells us. It is this fact which introduces the difficulty. If we did observe displacements of the synaptic knob correlated with what the subject says concerning the use of his will, the supporters of hypothesis (*b*) might argue that this is so simply because both the knob displacements and the subject's statements result from the same antecedent neural events occurring in his brain without any intervention of mind or will. In any case, since we could hardly study the synaptic knob in isolation from the rest of the brain, the supporters of (*b*) would generally be able to suggest that any unexplained displacements of it are due to the intervention of insufficiently-known influences of purely neural, material origins. We would thus be driven to the task of testing whether the behaviour of all the individual elements of a considerable part, or of the whole, of the brain obeys the ordinary ' laws ' of physics—which would prove hypothesis (*b*)—or whether these elements have a behaviour which is partly ' outside physics '—which would prove that hypothesis (*a*), or some similar interactionist theory, is true.

This would appear to be an overwhelmingly difficult task. The author may have in mind some special method of disproving (*b*) in spite of difficulties, but at first sight it does indeed seem doubtful whether this hypothesis could be disproved by experiments.

At this point I may perhaps say that I personally believe that, taken as expressing the whole truth about man, the mechanistic hypothesis (*b*) is false. I believe it to be false because I attach great importance to the sense of freedom of will that I have, and I hold, despite arguments to the contrary, that this is in fact an index of genuine freedom of will. Nevertheless, it seems to me that hypothesis (*b*) constitutes the natural working hypothesis to be used in physiological investigation, for the following reasons.

If a student of neurophysiology were to assume from the start that hypothesis (*b*) is false and that (*a*) is true he would be able, without further inquiry, to explain most of his experimental observations in terms of the intervention of ' mind influences '—rather as some aeroplane pilots were said to ascribe unexplained mechanical trouble in their machines to the intervention of ' gremlins '. The way to test hypothesis (*b*) therefore is to use (*b*) itself as a working hypothesis in the hope that, if it is both untrue and testable, it will lead one into predictions contradicted by experiment. If, however, hypothesis (*b*) were not testable by experiments, the investigator would be able to make uninterrupted progress in his experimental work whilst using it, so that, true or untrue, (*b*) would clearly be a suitable working hypothesis.

Thus it seems that, whether hypothesis (*b*) is true or false, testable or untestable, a physiologist may logically be led to use it as a working hypothesis, at any rate as long as there is not convincing experimental evidence to prove it false. In this sense, this purely neural, mechanistic and materialistic hypothesis may be said to constitute the heuristic postulate of neurophysiology, even though many do think that, from a more general, philosophical, standpoint it is untenable. The use of this hypothesis does not necessarily imply that its user believes it to give a true, complete picture of the nature of man.

The permissibility of using hypothesis (*b*) is of particular importance in sensory physiology in the case of certain experiments performed on conscious and ' intact ' human subjects, for instance in the study of colour vision. The theory of such experiments—which is not discussed in great detail by the author—often involves the notion of a chain of reactions the extreme links of which are sensory receptors at one end and effector mechanisms at the other. Now if such a chain includes mental links, this generally leaves so much scope for speculation on the actual events occurring in these links that various conflicting interpretations can be put forward and different theories in unrestricted numbers are equally possible. For instance one has been led to use in colour vision the concept of the mental ' mixture of elementary sensations ' but it has never been possible to make a satisfying choice among the many possible ways in which such a ' mixture ' might take place. Conversely, important advances in the physiology of human colour vision have recently been achieved by investigations which dispense with such mental concepts whilst making implicit use of hypothesis (*b*).

It may be pointed out here that in many experiments in sensory physiology performed on human subjects the aim of the experimenter is evidently to make the subject respond in as reproducible a manner as possible. The ideal would be to induce the subject to respond with the regularity and reliability of a piece of physical apparatus of the most stable kind. Indeed the performance of certain subjects sometimes comes near such an ideal. When this occurs it is apparently due to a large extent to some particular effort of mental concentration on the part of the subjects. Thus it seems that it is an act of will which here leads to machine-like reliability in the subject's behaviour. The result of the intervention of will, it would appear, is precisely to set the nervous system into such a condition that it comes near to working like an automatic detector of physical stimuli. It might possibly be said that the subject strives consciously to achieve something akin to reflex behaviour. According to such a speculation, the mechanistic hypothesis (*b*) might actually correspond to a true description of such experiments in so far as the processes *directly* involved in the experiments are concerned ; these would in fact be purely neural mechanistic processes.

The will would be concerned in setting up this particular state of affairs in the nervous system but would not intervene directly in each response given by the subject. Again the nervous reactions which are supposed to lead to conscious perception of the stimulus might occur as it were in side lines and thus play no direct part in the primary processes strictly relevant to the experiments. This is only speculation, but it shows that even within the framework of the author's theory it may sometimes be permissible to leave out of consideration purely mental events in the theoretical explanation of sensory processes occurring in fully conscious human subjects. It also shows incidentally that the detection of the ' mind influences ' of the author's theory will become very complicated if these influences do act upon the nervous system in such devious manners.

The preceding remarks give only an indication of the problems raised by the book under review. Many other topics call for discussion which cannot be studied here. The publication of further advances of the author's theory will be awaited with keen interest.

In conclusion, Professor Eccles's book will be of great value to those who wish to study neurophysiology and will also be found most stimulating by those interested in the wider problems, whether of ' science ' or of ' philosophy ', arising from the study of neurophysiology.

M. H. Pirenne

INDETERMINISM

This volume[1] contains two sets of papers previously published by the author, between the years 1924 to 1927 and 1951 to 1952, with added comments ; an introductory essay explains the history and present state of quantum-mechanical problems. An article on the relation of relativistic to quantum physics, together with two short notes which appeared originally in the *Comptes Rendus*, by M Vigier completes the volume.

Today, after more than twenty-five years, the basic interpretation of quantum mechanics is again under discussion. There are several reasons for this. The great success of quantum physics in the atomic domain was followed by a comparative failure in the sub-atomic, or nuclear, domain. Moreover, Einstein's quiet but persistent criticism, as well as the recent papers by D. Bohm, have revived the interest in fundamental problems. As it appears now, these problems arise at bottom from the difficulty of combining the concept of field with that of particle.

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[1] *La Physique quantique restera-t-elle indéterministe ? Exposé du problème, suivi de la reproduction de certains documents et d'une contribution de M Jean-Pierre Vigier,* Louis de Broglie (Gautier-Villars, Paris, 1953. Pp. vii+111. 1500 fr.)

M de Broglie begins by tracing the development of his ideas. Inspired by relativity theory, he associated with the rectilinear and uniform motion of a particle (in field-free space) a plane monochromatic wave with constant amplitude and linear phase. While the energy and momentum of a particle are related to the frequency and wavelength, its motion is described by the wave phase. Since such a wave has the same amplitude everywhere in space, how can we determine the position of the particle ? M de Broglie concluded that the particle is found anywhere with the same, *a priori*, probability and that the amplitude does not give the *true* position of the particle. Therefore, he called the wave a ' phase wave ' in order to indicate that the phase alone has a physical meaning.

In the early days of quantum mechanics three interpretations of the wave-particle dualism were considered : the double solution, the pilot-wave, and the interpretation in terms of complementarity.

M de Broglie originally took the particle to play the role of a singularity within the wave. The continuous ' ψ ' wave is, after all, quite successful, and so a second wave ' u ' is assumed containing the singularity. Both solutions have the same phase (in x, y, z, t) though they differ in their amplitudes—one being constant and the other a function of time. It can then be shown that the singularity, in the course of time, describes a path so that its velocity at every point is proportional to the gradient of the phase. The possible trajectories of the point-singularity thus coincide with the set of curves at right angles to the surfaces of constant phase of ψ. Under these conditions it can be proved that the probability of finding a particle at any given point is proportional to the square of the amplitude of ψ. This is required for explaining electron diffraction and is, in fact, no more than a principle of ordinary optics, in conjunction with Einstein's photon hypothesis.

In M de Broglie's view, only the u-wave and not the ψ-wave is *real*. The impression that the ψ-wave alone suffices for describing our experiments is erroneous : it is created by the postulate of the *concordance of phase*. This is the key to the ' subtle and curious ' interpretation which was proposed by the author in 1927.

On the basis of the double solution it is possible to derive the ' guide formula ' describing the motion of the particle as a function of the gradient of the phase common to both waves. In the commentary added to the original paper M de Broglie improves on the derivation by dropping the assumption, previously made, that the velocity of the particle at any point must have the same direction as the gradient of the phase at that point. This ' pilot-wave ' interpretation was reported on by the author at the Solvay Congress (1927). Today he does not think that it can ever become acceptable, even though the theory of the double solution may again play a rôle. For the postulate of *concordance* (on which the double solution is based) can be so weakened that the two phases, and their first derivatives, need only have the same value for a small sphere around the singularity. There is hope, then, that the postulate may find a mathematical justification.

Similarly, M de Broglie feels now that the wave with singularity cannot be a solution of a linear equation as originally assumed. The equation of motion of the particle must resemble the (quadratic) equation of general relativity and need be linear only in approximation. The values of *u* would have to be large in a small region around the singularity ($\sim 10^{-13}$ cm.), while outside of it the linear approximation may hold. According to the author, however, the outlook is not too hopeful. For the solutions of non-linear equations are, mathematically, even more difficult than the solutions with singularity of a linear equation.

Following these ideas, M Vigier attempts to join quantum mechanics with general relativity by an analogy between the double solution with a theorem of Einstein's. Taking the particle as a singularity in the gravitational field, Einstein has proved, from the field equations alone, that the singularities move along the geodesics in space-time. This is similar to M de Broglie's guide formula, i.e. that the particle moves along the gradient of the phase of the *u*-wave of which it constitutes the singularity. This is certainly an interesting proposal ; however, no definite results have so far been achieved.

As everybody knows, the probability interpretation emerged victorious in the Solvay discussions. Since that time, M de Broglie—abandoning his early ideas—has accepted and loyally defended the statistical view. But today quantum mechanics is in danger of becoming sterile. Is this the fault of the ' orthodox ' interpretation ?

The objections advanced here may be summarised under four headings. (1) The concept of probability as used in quantum mechanics differs from that in, say, kinetic theory and, equally, from the ' classical ' standard set by Laplace and Poincaré. For in quantum physics we have a ' pure ' probability while, ordinarily, probability results from the merely technical, and not essential, *ignorance* of the parameters describing an underlying, *deterministic*, process.

But can such an argument really be maintained ? The mathematical calculus allows us to compute probabilities only from other probabilities initially given. Probability is a concept *sui generis*, even though we may use it on occasion when other, and more preferred, methods fail. We interpret kinetic theory by the classical particle model ; we still need the *ergodic theorem* (or one of its variants) in order to assign equal probability to every molecule (with respect to the energy) in the equilibrium state ; otherwise we cannot strictly derive, e.g. the Maxwell-Boltzmann distribution. Similarly, in the classical examples from dicing, we do know that the die is symmetrical, etc. ; if it is not, or if the die turns out to be loaded, we act on this knowledge and change our bets. In other words, probability in quantum mechanics is the same as it is everywhere else : what is different is the obsolete, and scientifically irrelevant, ontology behind the various theories in which probability is applied.

(2) M de Broglie then proceeds to criticise von Neumann's proof that quantum mechanics, in its statistical interpretation, does not contain any hidden parameters. He argues that the theories of the double solution and of the pilot-wave, though they cannot be taken as proved, nevertheless do exist. Can this be reconciled with von Neumann's proof? Surely, there is a misunderstanding : a completeness proof in the Hilbert sense (which is in question here) does not imply that there may not be more than one interpretation for a certain calculus, nor that one of the possible interpretations may not contain hidden parameters. Obviously, one can prove the completeness of a physical, that is, interpreted, theory only if one agrees beforehand on the interpretation. For this reason also it is fallacious to argue that the probability distributions in quantum mechanics *exist* before an experiment is made by which their specific values may be found. It depends of course on what is meant by ' existence '. But we need not commit ourselves here to any philosophical view ; it is enough to say that all the quantities involved are statistical, that is, to specify their initial distribution. Then it can be proved that no other parameters than those given are needed. This is not as trivial as it sounds : it shows—like the proofs of this kind in mathematics—that the statistical theory of quantum mechanics is basically complete. This cannot be said of any of the other interpretations proposed ; but it does not exclude the possibility that different, and perhaps better, interpretations may not be found one day.

(3) Einstein's well-known objections are then described, i.e. that quantum mechanics violates the principle of contact-action. The ' causal anomalies ' have been discussed in detail by H. Reichenbach (*Philosophic Foundations of Quantum Mechanics*, University of California Press, 1944). And the rôle the principle plays in classical and quantum physics has been described in this *Journal* (E. H. Hutten, 1951, **2**, 45). I would like, however, to add the following comment. In a diffraction experiment, for example, the ψ-wave front is extended over the whole, macroscopic, space behind the slit. Therefore, the probability of finding the electron is the same everywhere ; but when the electron hits the photographic plate, the wave is suddenly reduced to a point, and the probability of finding the electron anywhere else has become zero. This shows indeed that action-by-contact which represents the ' causal mechanism ' in classical as well as in relativity physics no longer applies. One can also say that the concepts of wave and particle, though they originate in ordinary experience and are used in classical physics in a *slightly* different manner from everyday discourse, are used in a *more* different way in quantum physics. One may say that the classical (and relativistic) concepts of space and time appear to be in need of revision

in the microscopic domain. But can one argue, as M de Broglie seems to do, that quantum mechanics forces us to accept the breakdown of the space-time framework *even in the classical domain*, because the photographic film is of macroscopic dimensions ?

It is, however, strange and somewhat inexplicable that special relativity can be successfully applied in quantum mechanics, e.g. for determining the fine structure constant and the electron spin. For relativity theory is classical in spirit ; it is based on the continuum idea and the principle of contact-action. In M de Broglie's view, it is therefore paradoxical that *general* relativity seems to be so completely separate from quantum theory. In one sense, i.e. of *correspondence*, this is not so. The quantum-mechanical equation of motion, like the relativistic equation, both lead to Newton's equation in first approximation. Is it not possible that, starting from Newtonian mechanics, we go to the microscopic domain with quantum mechanics, and to the megaloscopic domain with relativity, and that the two meet only at the juncture ? Can any other kind of ' synthesis ' be expected, however much we might desire it ?

(4) The problems raised by quantum mechanics are not simply physical, that is, they cannot be immediately decided by experiment. The problems are also philosophical, or logical, for they depend largely on our way of speaking. Or, more precisely, they have a *semantic* aspect, since we argue, on the basis of experiments, what *meaning* the quantum-mechanical concepts and expressions have and how this meaning differs from that of similar expressions in classical physics.

This is also shown by the fact that typically philosophic terms such as ' existence ', ' reality ', and ' knowledge ' continue to crop up in the discussion. Naturally, the terms of reference one may accept depend on many, and often personal, factors. In science we aim at minimising our private preferences and at taking success in experimentation as a criterion : this is sometimes easier said than done. But let us not overlook the decisions implicit in the choice of language we have made. If ordinary, three-dimensional, space alone is ' real ', then of course configuration space becomes ' fictitious '. If ' wave ' and ' particle ' are taken in the classical sense, then there is something wrong with them when interpreted in terms of complementarity. If the causal mechanism with which we endow macroscopic processes is taken as standard, then microscopic processes are ' acausal ' : though we still have laws (even of the classical, mathematical, form) allowing us to describe our experiments and to predict their outcome. If ' to know ' means ' to be able to determine the p's and q's with unlimited accuracy ', then our knowledge of quantum phenomena is incomplete ; or, we must say that the probability distribution of p, q is ' created ' by the act of measurement ; or, that the observer ' disturbs ' the natural course of events, and so on.

But we need not speak in this way and can still avoid the somewhat naïve positivism of the earlier formulations of quantum mechanics. We must only be aware of the fact that with our customary language we introduce, unwittingly perhaps, a *classical* bias. We speak of '*in*determinism', '*un*certainty', etc. and so suggest that there is a defect somewhere. M Vigier points out, I think rightly, that our theories have not only an experimental, but also a logical limit. The dream of Descartes which both authors invoke is very beautiful: but can we never have any other?

These essays are an example of 'tout ce que n'est pas clair n'est pas français'. The volume is the first of a series entitled *Les grands problèmes des sciences.* The editor, Mme Destouches-Février, must be congratulated on so auspicious a beginning.

E. H. Hutten

A New Approach to Psychical Research, Antony Flew (Watts, London, 1953. Pp. vii + 161. 10s. 6d.)

In this work, ostensibly intended for the layman, but written with one eye on the philosopher, Mr Flew attempts the well-nigh impossible task of covering in a compass of 161 pages a great many aspects of that complex medley of claims which is known as psychical research. The author is of the opinion that neither the spontaneous cases alone nor even the so-called mediumistic phenomena are sufficient to establish the existence of a paranormal factor without the confirmation of the experimental work in extra-sensory perception. It is therefore surprising that he has devoted so little space to the card-guessing experiments, though this may be due to exasperating limitations apparently imposed upon him by his publishers.

The result is that the crucial Pearce-Pratt series is not mentioned while the important work of Humphrey and Schmeidler on the correlation of personality factors with ESP performance is accorded just one half sentence! There is a more reasonable description of the Shackleton experiments and of the work of Whately Carington.

However, there is little doubt that Mr Flew would consider the chapter on human survival and that entitled 'Describing and Explaining' as the *raison d'être* of his book. The interpretation of the mental phenomena of mediums as examples of ESP among the living rather than as communications from surviving entities, however, invokes arguments that have been long familiar to psychical researchers and contains little that is novel.

The chapter on 'Describing and Explaining', however, puts forward views of considerable interest with many of which the present reviewer is unable to agree.

Mr Flew thinks that orthodox science need not worry unduly over the fact of ESP which seems to contradict some of the fundamental axioms of modern physics such as that of spatio-temporal continuity and the commonly accepted principle of causation, since these extra-sensory phenomena may be regarded as rare exceptions which do not conform to the general rules. That is, they represent unimportant aspects of the universe, which do not fit in to the ordinary framework of science. But, as C. W. K. Mundle has remarked, if science had adopted this attitude when the study of electromagnetic phenomena was in its infancy and regarded them as merely an unimportant exception to ordinary Newtonian mechanics and hence not worth bothering about, the world would have been a vastly different place today.

Perhaps Mr Flew's most interesting suggestion concerns precognition. If this phenomenon becomes in due time well-established we may have to substitute for the ordinary notion of cause a modification of the hypothesis of *mnemic causation* advocated by Bertrand Russell. The difficulties of a mechanical theory of memory have not so far been resolved and theories which postulate, for instance, permanent circulatory currents in the neural network are open to many objections. This led Russell to postulate that a proximate cause of a memory was not a present event but a present event together with a past event. In the case of precognition we should have to consider a cause as a combination of a present and a future event.

On page 118 there is the perplexing statement that ' psi-gamma ' (a non-committal term used for extra-sensory perception) can be defined only as ' the factor which gives rise to significant deviations from mean chance expectation *in a series of guesses* '. Flew goes on to say that it is *meaningless* to *ask* whether any single correct item is an example of ESP. Now this seems to us unnecessarily sophisticated. It is perfectly true that at a *conscious level* card-guessers cannot distinguish between guesses which are ' hits ' on the target and those that are misses. Therefore, at present, we are unable to choose any one correct guess and decide that ESP and not chance was responsible for it. But a study of certain position effects affords us reason for supposing that the guesser is often aware of having scored a success at *some subconscious level* for there is some evidence that the ' hit ' may cause him to modify his ordinary guessing pattern.

Mr Flew's view that paranormal mental phenomena have been demonstrated only in a statistical sense must surely involve him in difficulties with regard to the spontaneous and mediumistic cases, which, be it noted, he regards as being established by reference to the strictly experimental work.

It may be a misuse of language to regard ESP as a form of ' cognition ' or of ' perception ', but Mr Flew's model for it as a form of ' guess work ' has little to recommend it since it does not help us in any way.

Scattered through the book are several references to the attempts of G. Spencer Brown to explain away ESP as a statistical artifact, but until Mr Brown's findings have been published and discussed it seems somewhat premature for Mr Flew to hint (p. 129) at a possible ' Copernican revolution in the interpretation ' (of ESP), more especially after his conclusion (p. 93) that it seems ' impossible to interpret the results (of the Shackleton experiments) as a discovery about statistics rather than a discovery about a rare human faculty '.

There are two statements in the book which it would be charitable to assume have escaped revision by the author. On page 88 we read that ' steady scoring (on five symbol cards) of six or seven right out of twenty-five is very significant indeed and the odds against it occurring by chance alone are monstrous '. But surely the degree of significance depends on the number of runs of twenty-five over which the given average is maintained. And on page 114 occurs a sentence which will make physicists raise their eyebrows. ' For instance, all radiative effects have an intensity proportionate to the distance from source, whereas nothing like this has been observed with ESP '.

To sum up there is not a great deal that is really new in Mr Flew's ' new approach ' though there are some stimulating suggestions and some linguistic arguments, with an air of sophistication which sometimes turns out on closer examination, to be no more than superficiality in disguise.

S. G. Soal

An Essay in Modal Logic, Georg H. von Wright (North Holland Publishing Company, Amsterdam. Pp. 90. 15s.)

In this monograph Professor von Wright gives descriptive sketches of several systems of modal logic. The main interest of his work lies in the fact that the systems are related in interesting ways by analogies between the various modal concepts with which he deals. He distinguishes four classes of such concepts. The first class, which he calls *alethic* modalities or modes of truth are the familiar concepts of necessity, possibility, contingency and impossibility. Von Wright further distinguishes *epistemic* modalities (verified, undecided and falsified), *deontic* modalities (obligatory, permitted, indifferent and forbidden) and *existential* modalities (universal, existing and empty). This last class of concepts is, as von Wright points out, normally the concern of quantification theory and not of modal logic.

The relations of analogy between the members of these four classes of modal concepts are illustrated schematically by von Wright in the following table where the modes on the same horizontal line are analogues of each other :

alethic	*epistemic*	*deontic*	*existential*
necessary	verified	obligatory	universal
possible	—	permitted	existing
contingent	undecided	indifferent	—
impossible	falsified	forbidden	empty

A further distinction is introduced in the case of the alethic and the epistemic modalities. Modal statements of these kinds may be made *de dicto* (as when we say of a certain statement that it is necessarily or possibly true) or *de re* (as when we say that a certain property is necessarily or possibly present in a certain thing).

The exposition of these systems displays von Wright's usual impeccable standards of elegant technical accomplishment. He omits any development of the existential modalities and gives only a brief sketch of the deontic modalities. The rest of the book is devoted to explaining, in some detail, two systems of alethic modalities and two corresponding systems of epistemic modalities, three systems of combined epistemic and alethic modalities and, in outline, some systems of 'higher order' alethic modalities *de dicto*. These 'higher order' systems concern expressions in which modal operators are prefixed to propositions already containing modal operators. (' It is possible that *p* is necessary ' and so on.) Decision procedures by truth-table methods are given for these systems.

The monograph ends with two appendices, one giving a very useful decision procedure by means of Lewis Carroll diagrams for modal syllogisms and the other sketching the axiomatic basis for systems of alethic modalities.

The only controversial part of von Wright's work lies in the philosophical presuppositions of his logic. No doubt it is quite impossible for any logical systems to be quite free from such presuppositions. It is, however, worth remarking that the original features of his basic concepts (the multiplication of modal categories and the postulated analogies between them) and the distinction borrowed from medieval logic between statements *de dicto* and *de re* are not purely logical innovations. They raise philosophical problems and call for philosophical justification, though such discussion would no doubt be out of place in this book.

D. J. O'Connor

Prints and Visual Communication, William M. Ivins, Jr. (Routledge and Kegan Paul, London, 1953. Pp. xxv + 190. £2 2s.)

This lively and provocative history of picture-printing (from woodcuts to half-tone blocks) reproduces a course of eight lectures delivered by the Emeritus Curator of Prints of the New York Metropolitan Museum at the Lowell Institute in Boston. With refreshing unconventionality the author brushes aside the traditional aesthetic and bibliophile approach to old prints and illustrations. His problem is not the development of an art form but the evolution of a technique of conveying information. In the course of his survey the author treats the reader to a most stimulating analysis of the different functions of verbal and visual symbols. His description of the pictorial conventions used by sixteenth-century engravers to represent the visible world is a model of its kind. But the author aims beyond descriptive analysis to an explanation of the progress of Western thought in terms of his dichotomy between verbal and visual ' communication '. Unfortunately it is precisely this aspect (which must be of the greatest interest to readers of this *Journal*) which is least acceptable. The author is so deeply convinced of the inadequacy of words as scientific tools that he would like to attribute almost the whole technological and scientific progress of the Western world to the invention of making ' exactly repeatable pictorial statements ', that is, to prints and photography. ' Science in actual practice is not a dead body of acquired information but an actively growing accumulation of hypotheses put forth to be tried and tested by many people. This trying and testing cannot be done without repeatability of communication.' This challenging thesis is particularly developed with respect to the alleged limitations of science in classical antiquity. The Greeks and Romans, we learn, were thrown back on verbal description and thus tempted to develop a predominantly literary and rhetorical culture, the one-sidedness of which comes in for some merited and some unmerited ridicule on the part of the author. The *pièce de resistance* in this historical argument, which is somewhat parallel to that of Zilsel and Farrington, is a passage from the Elder Pliny's *Natural History* (xxv, 4) referring to the difficulties of botanical illustration. Since the author (slightly flaunting his inability to read the original) relies on a rather inadequate translation it may here be reproduced in Latin : ' Verum et pictura fallax est coloribus, tam numerosis praesertim in aemulatione naturae, multumque degenerat transcribentium fors varia. Praeterea parum est singulas earum aetates pingi, cum quadripertitis varietatibus anni faciem mutent.' The difficulty to which Pliny refers, of rendering the exact hues and varying colours of a species during the four seasons and preserving it in subsequent copies is no doubt a real one. But Mr Ivins appears to underrate the degree to which it was surmounted. Historians of book illuminations have long come to recognise that herbals, bestiaries, astronomical and mechanical texts tend most faithfully to preserve the style and character of their illustrations simply because their purpose demanded a 'facsimile' reproduction. Moreover, the main problem which Pliny mentions, that of colour, has not even been solved today. But has this technical difficulty really impeded the progress of botanical taxonomy? Was it not rather overcome by selecting criteria (such as Linnaeus chose) which can be described quite unambiguously? After all, illustrations can only be more or less accurate. Only propositions can be true or false, and without this distinction there can be no science.

It is in trying to tackle these philosophical questions that the author seems to overtax the relevance of visual communication. To him almost the whole of classical philosophy seems due to an enforced reliance on verbal rather than visual symbols. Plato's theory of ideas no less than Aristotle's doctrine of substance and accidents might have proved unnecessary, he implies, if these philosophers could have understood that not only verbal counters for universals but also descriptive pictures of particulars are 'repeatable'. The author seems to have overlooked the fact that the principle of making repeatable images (as distinct from prints) was extremely familiar to the ancient world wherever seals were used. It so happens that this very principle of stamping an image on wax or another substance is one of the most important models Plato uses to explain both his theory of ideas and his conception of memory traces. Philosophically, therefore, he could have learned nothing from the print. That printing helped a great deal in the spreading of information is incontestable but even here the author tends to overlook rival means of duplicating forms; his caustic remarks on eighteenth-century critics deriving their knowledge of classical statues from falsifying engravings should have been tempered by some reference to the rôle of plaster casts.

It is always amusing to watch Mr Ivins grinding his axe, but where he grinds it too fine it will not cut ice. After all, the technique of making repeatable pictorial statements, i.e. the woodcut, was known to the Chinese long before it was applied in Europe, without giving the Far East a corresponding advantage over the West. Perhaps there is some leaven in the Classical heritage which accounts for this difference? For though the author would hardly be ready to concede the point, the bracing questioning spirit in which his own book is written owes more to the verbal culture of the Greeks than it does to any technique of 'doing things'

E. H. Gombrich

Introduction to Logical Theory, P. F. Strawson (Methuen, London, 1952. Pp. 266. 15s.)

The main purpose of the author is to compare the logical pattern of ordinary language to that of 'artificial' language, i.e. of deductive systems. Logic, in its usual sense, is presented briefly and in a somewhat simplified manner (truth-functions, classes, predicative formulae, and the syllogism), but always with the original purpose in mind. Ordinary usage is analysed here by means of suitably chosen examples, and the analysis is based upon the concept of *entailment*. In this way the author tries to show that there are two kinds of logic, that formal logic is of limited value only for our understanding of language, and that there is a fundamental rift between ordinary language and the language systems of science.

No one today denies that in our ordinary reasoning we often neither are, nor mean to be, logical. But if we wish to establish the *validity* of an inference, we must reconstruct our expressions in logical terms as closely as possible ; just as we cast certain scientific expressions into a mathematical form if we wish to make computations. Thus we are forced to draw boundaries and to make a decision ; and though the author is right in saying that our decisions are often neither conscious nor purely verbal, they are still decisions and, in this sense, arbitrary. The method of examples is as selective as, say, the method of formalisation employed by the logician ; and the concept of entailment is no whit less technical than that of implication or than any other concept of standard logic.

It is of course true that our standard schemes do not always fit as well as we might desire. This may mean one of two things : either our logic is inadequate (but, then, why not try to develop it a little more ?) ; or the expressions chosen for reconstruction are too vague and their meaning or, rather, their meanings must first be made clearer. The author appears to favour the first alternative while overlooking, if not rejecting, the second, i.e. that meaning presents a problem, even in formal logic.

This weakness is inherent in ' the natural language approach '. There is the tendency to forget that *all* usage, even ordinary usage, is based upon whatever information the user possesses ; and that ordinary usage often embodies mistaken, or out-of-date, information. Modern logicians have long recognised that the syntax of a language cannot be studied without regard to its semantics ; even a formal system has an interpretation, i.e. in terms of the logical constants chosen. It is certainly impossible to say that logical expressions, in contrast to ordinary sentences, are *independent* of the context of their use. And if logic deals with true statements, how can we do without the semantic conception of truth ? No wonder that the author has to invent a ' second ' kind of logic in which the entailment-rules (as he calls them) are supplemented by referring rules. The narrow, and rather old-fashioned, view of logic the author adopts entails one of the epistemological theories of truth.

By dispelling the myth that logic is an ' ideal ' system of prescriptive rules for a ' universal ' language the author has gained a quixotic victory ; instead, he sets up the opposite myth that ordinary usage and natural language are the standard by which logical reasoning is to be judged. This is another romantic ideal reminiscent of the 'natural man' or the 'noble savage'. The dichotomy between ordinary and artificial language, and between 'natural' and formal logic, is false. There is a continuous range of linguistic usage, from the most ordinary to the most technical; just as there are many levels of explanations for this usage, from the common sense analysis to semantics.

Some of the explanations given here, both of ordinary usage and of logical rules, are—in my opinion—doubtful but they are always suggestive and important. We are rightly reminded that not all our ordinary arguments can be reconstructed in logical terms, e.g. the subjunctive conditional. The book is valuable as a stimulus for continuing the discussion of the logic, as well as of the grammar, of language.

E. H. HUTTEN.

Brett's History of Psychology, edited and abridged by R. S. Peters (George Allen and Unwin, London, 1953. Pp. 742. 42s.)

ATTENTION should be drawn to the methodological interest of this edition. In making his abridgment the editor has interpolated connective passages which are printed in type that is smaller than that of Brett's text. These contain excellent methodological discussions of Bacon and Descartes. He has added a useful introduction and a long and valuable concluding chapter on twentieth-century trends. However interested in Brett's text readers may be, this last chapter is bound to have a wider appeal. It is notable for two things. One is the account of the chief modern schools of psychology. Perhaps as a result the word 'behaviourism' will be bandied about less indiscriminately. Again, the account given of Freud is one of the most coherent that has appeared. It is highly accurate except on one point—the author misses the factors that led Freud to give the concept of sex an apparently new and misleading scope. Dr Peters successfully relates Freud's theories to those of other psychologists, and is able to talk in the same breath of Freud and McDougall and yet talk sensibly. The other notable feature of the chapter is the order Dr Peters manages to impose on the welter of modern schools of psychology (without, however, distorting them). This he achieves by assembling the material under methodological groupings. He is able to describe the relationships between opposing schools, for example, by depicting their attitudes to observationalism. Thus the author succeeds in illustrating very clearly how a body of psychological theory can stem to a highly significant extent from a methodological theory.

Dr Peters tells us that he has adopted the methodological views of Professor Popper ; and it is indeed obvious from his handling of the subject that he has in fact done so. It is of interest, moreover, to see Popper's methodological views applied in this field.

J. O. Wisdom

Sir William Osler: Aphorisms from his Bedside Teachings and Writings, collected by Robert Bennett Bean ; edited by William Bennett Bean (Henry Schuman, New York, 1950. Pp. 159. $2.50.)

Maxims, aphorisms, epigrams, apothegms : presumably these are words with different shades of meaning ; certainly the contexts in which we should think it apt to use them are quite distinct. Such a rich synonymy or near synonymy should be earned by only the most highly treasured instruments of human communication—or so one would suppose. But in reality, it is not so. Sententiousness, like punning, has had its day, and conversation nowadays goes at too quick a tempo to take delivery of aphorisms. The invoice alone must suffice. So aphorisms have been put to new uses. Taken before retiring, ten aphorisms from (say) Logan Pearsall Smith's compendium are a sedative dose, and against ten more the most obstinate wakefulness cannot prevail.

But let us suppose that an aphorism is the expression, complete and rounded in itself, of an original, definitive and arresting thought that embodies some unrecognised or dimly credited truth. How many thoughts of such a kind, eligible for such a form of expression, can any one of us lay claim to in a lifetime ? The answer is, precious few. Now Sir William Osler was a great physician (many think the greatest of his day), but in this little book are gathered together no less than 356. Dissolved in such a volume, would it not be a miracle if the thought were not dilute ? To be fair to Osler, he cannot have intended that much of what is set out here should have been presented in the aphoristic style : that is the work of other, well-meaning, hands. ' Cultivate your powers of observation ' (9) ; ' education is a lifelong process ' (15) ; ' live a simple and temperate life ' (94) ; ' to get a breadth of view . . . is difficult without travel ' (141) ; ' the physician needs a clear head and a kind heart ' (144) ; ' we are here to add what we can to, not to get what we can from, Life ' (154) ;—do we need Osler's assurance of the truth of these ? Osler reveals his power and wit only when he speaks as the great professional clinician that he was. ' Never forget to look at the back of a patient. Always look at the feet. Looking at a woman's legs has often saved her life ' (204) ; ' one of the first duties of the physician is to educate the masses not to take medicine' (209); 'the young physician starts life with twenty drugs for each disease, and the old physician ends life with one drug for twenty diseases' (253). But the real measure of Osler's importance in medicine is the number of his sayings that now seem platitudinous because his teaching has made them so. 'There can be no doubt of the value to the physician of a very thorough training in methods and ways of organic chemistry' (23); 'the student needs more time for quiet study, fewer classes, fewer lectures' (47); 'there are, in truth, no specialities in medicine, since to know . . . diseases a man must be familiar with their manifestations in many organs' (74). These are not aphorisms, of course; they are simply sayings that needed to be said.

P. B. Medawar

Francis Bacon. A Bibliography of his Works and of Baconiana to the year 1750, R. W. Gibson (Scrivener Press, Oxford, 1950. Pp. xx + 369. £7 7s.)

The name of Francis Bacon has for so long been a symbol, first of the most penetrating analysis of the methods of 'inductive science' and later of the most obtuse misunderstanding of them, that readers of this *Journal* cannot fail to be interested in the assemblage of data, presented in this book, showing the prodigious reproduction and spread of his writings during the century-and-a-half after their first publication.

To what extent was Bacon's enormous posthumous reputation as a logician of science justified? This is a thorny question, from which few have emerged unscratched who have ventured into it. The main points of criticism have been that Bacon misunderstood the rôles of hypotheses and of mathematics in scientific explanation. Compared with Galileo on these matters Bacon certainly comes off badly. But the modern logician of science can still turn with profit to the contributions for which he was valued in the seventeenth century: his classic descriptions of the analytical process of sorting out and classifying the elements of a problem before framing a hypothesis, and of the experimental elimination of false hypotheses. Bacon's well-known study of heat suffers as much from dealing with a subject which the physics of his time (and for some century and a half afterwards) could not incorporate, as from his own treatment of it, though his selection of this problem as an example of his method would alone prove that he was no Galileo. But his account of the 'inductive' analysis of a problem is of considerably more interest than some recent critics of J. S. Mill would have us believe. Dr Gibson is to be congratulated on this learned bibliography of Bacon's writings and influence, and the Scrivener Press on the fine reproductions of the rich and elegant title pages, in which Bacon's works are seen to have been so fortunate.

A. C. Crombie

Descartes' Philosophical Writings, selected and translated by Norman Kemp Smith (Macmillan & Co., London, 1952. Pp. viii + 317. 15s.)

This book contains several of Descartes's works that have already appeared in English, such as the *Discourse*, *Meditations*, and the theory of vision as expounded in the *Dioptric* ; the new translation was rendered necessary by recent research which has thrown light on the meaning of certain key terms. What will chiefly interest readers of this *Journal* is the small collection of writings on the mind-body problem, including a notable letter to Princess Elizabeth. Descartes considers that ' the body has all the dispositions requisite for receiving the soul, and without it is not properly the human body ' ; moreover, the only notion we have of the soul and body in combination is the notion of *union* ; and ' the things which pertain to the union of soul and body . . . are known very clearly by the senses '. This is very unlike the Descartes we are brought up on ; indeed, he seems to have been in a sense a monist : ' to apprehend the union of two things is to apprehend them as one single thing '. He was concerned also to understand how the soul could move the body : he denies that for one thing to move another they must have surfaces in contact ; for weight is a moving force that does not act in this way : ' it is no more difficult for us to apprehend how the mind moves the body than it is for them [the majority of philosophers] to apprehend how such a quality carries the stone downwards '. Moreover, gravity could exercise its force at any point of a body even though a rope supporting it might be attached to quite different places. What he is pointing out is that, though physical properties are not all localised, they can have a physical effect ; and he is using this fact as a model for the action of the mind on the body. These passages are most stimulating and interesting.

J. O. Wisdom

ANNOUNCEMENTS

Prize Essay on 'The Age of the Universe'

(See Announcement in this *Journal,* 1953, **4**, 92)

THE Judges of this Competition (Professors Sir Harold Jeffreys, F.R.S., F. A. Paneth, F.R.S., K. R. Popper, and Mr L. L. Whyte) have made their Report.

Twenty-six Essays under pseudonyms were received from eight countries. One of these arrived over a month late and was excluded.

The Prize of £50 is divided. A First Prize of £30 is awarded to Mr Michael Scriven (Department of Philosophy, University of Minnesota, Minneapolis, U.S.A.), and a Second Prize of £20 to Dr J. T. Davies (Department of Physical Chemistry, King's College, London). Dr Ernst J. Öpik (Armagh Observatory, Armagh, Northern Ireland) and Dr G. J. Whitrow (Imperial College, London) receive honourable mention.

The November issue of this *Journal,* will be a special number devoted to 'The Age of the Universe'. It will contain a Report from the Judges, the two Prize Essays, and other Essays of special merit.

EDITORSHIP OF THE *JOURNAL*

Dr A. C. Crombie has resigned his position as Editor of the *Journal* on taking up his new post as Lecturer in the History of Science at the University of Oxford. He has been Editor of the *Journal* from its foundation, and we wish him every success in his new work. We are glad that he is willing to continue to serve as a member of the Editorial Committee.

Dr J. O. Wisdom has been appointed Editor.

Signed on behalf of the Editorial Committee,
L. L. WHYTE, *Chairman*

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

Enno Bartels, *Ludwig Klages seine Lebenslehre und der Vitalismus*, Westkulturverlag Anton Hain Meisenheim am Glan, 1953, pp. 113.

Irving M. Copi, *Symbolic Logic*, The Macmillan Company, New York, 1954, pp. xiii + 355, $5.00.

Donald Davidson, J. C. C. McKinsey and Patrick Suppes, *Outlines of a Formal Theory of Value*, **I**, 1954.

Louis De Broglie, *The Revolution in Physics: A Non-Mathematical Survey of Quanta*, Routledge and Kegan Paul, London, 1954, pp. xii + 310, 18s.

Heimo Dolch, *Kausalität im Verständnis des Theologen und der Begründer neuzeitlicher Physik*, Verlag Herder, Freibutg, 1954, pp. xi + 239, DM. 11.80.

Albert Einstein, *La Théorie de la Relativité Restreinte et Générale*, Gauthier-Villars, Editeur-Imprimeur-Librairie, Paris, 1954, pp. vi + 179, 1,300 fr.

Sigmund Freud, *The Origins of Psycho-Analysis*, Imago Publishing Company, London, pp. xi + 486, 1954, 30s.

W. B. Gallie and Others, *Uncertainty and Business Decisions*: *A Symposium*. The University Press of Liverpool, 1954, pp. viii + 104, 10s. 6d.

Erik Götlind, *Bertrand Russell's Theories of Causation*, Almquist and Wiksells Boktrycheri AB, Uppsala, 1952, pp. 164.

J. A. Hadfield, *Dreams and Nightmares*, Penguin Books, Harmondsworth, Middlesex, 1954, pp. xi + 244, 2s. 6d.

J. B. S. Haldane and Others, *New Biology: The Origin of Life*, Penguin Books, Harmondsworth, Middlesex, pp. 135, 2s.

Errol E. Harris, *Nature, Mind and Modern Science*, George Allen and Unwin, London, 1954, pp. xvi + 455, 35s.

Irving L. Horowitz, *Claude Helvetius; philosopher of democracy and enlightenment*, Paine-Whitman Publishers, New York, 1954, pp. 196, $3.00.

Julian Huxley and Others, *Evolution as a Process*, George Allen and Unwin, London, 1954, pp. 367, 25s.

Josef Mack, *Das Kosmische als Grundlage des Religiösen*, Westkulturverlag Anton Hain Meisenheim/Glan, 1953, pp. 118.

Dr Jakob Mandelker, *Matter, Energy, Mechanics*, Philosophical Library, New York, 1954, pp. ix + 73, $3.75.

F. S. C. Northrop and Mason W. Gross, *Alfred North Whitehead: An Anthology*, Cambridge University Press, 1954, pp. 928, 75s.

P. H. Nowell-Smith, *Ethics*, Penguin Books, Harmondsworth, Middlesex, pp. 224, 3s. 6d.

Kurt Reidemesiter, *Die Unsachlichkeit der Existenzphilosophie*, Springer-Verlag, Berlin, 1954, pp. 40, DM. 4.80.

Kurt Reidemeister, *Geist und Wirklichkeit*, Springer-Verlag, Berlin, 1953, pp. 92, DM. 8.60.

Minna Specht and Willi Eichler, *Leonard Nelson*, Verlag Offentliches Leben Frankfurt, 1953, pp. 305.

E. J. Trueblood, *The Dawn of the Post-Modern Era*, Philosophical Library, New York, 1954, pp. xii + 198, $3.75.

(*b*) JOURNALS

The contents of the latest issues of other journals are printed with the advertisements on the cover of this issue

Dialectica (Switzerland), Zurich, 1953, **7**, No. 4

F. Gonseth, ' Conférence d'ouverture '
P. Bernays, ' Einleitendes Referat '
H. Jecklin, ' Einleitendes Referat '
P. Nolfi, ' Eilneitendes Referat '
Th. Erismann, ' Wahrscheinlichkeit im Sein und Denken '
P. Nolfi, ' Über Ordnung und Unordnung (Diskussionsbeitrag zum Referat von Prof. Th. Erismann) '

Methodos, (Italy), Milan, 1953, **5**, No. 20

Vittorio Somenzi, ' Fisica e Logica '
L. Apostel, ' Logique et Preuve '

(*c*) ARTICLES

Juan David García Bacca, ' Sobre el fondo filosófico de algunas biologia matemática ', *Theoria*, 1952–53, **1**, 113

Juan David García Bacca, ' Historia filosófica de la Física, como serie de inventos conceptuales ', *Theoria*, 1953, **2**, 15

Juan Belgrano, ' Las matemáticas y la sistematización de las ciencias experimentales y de observación ', *Theoria*, 1952–53, **1**, 144

Alejandro Díez Blanco, ' Razón y experiencia en la ciencia de nuestros días ', *Theoria*, 1953, **2**, 87

H. Dingle, ' The Essential Elements in the Scientific Revolution of the 17th Century ', *Actes du Septième Congrès International d'Histoire des Sciences*, Jérusalem, Août, 1953, 272

H. Dingle, ' Science and Cosmology ', *Monthly Notices of the Royal Astronomical Society*, 1953, **113**, 393

A. C. Ewing, ' The Relation between Mind and Body as a Problem for the Philosopher ', *Philosophy*, 1954, **29**, 112

Jaime Echarri, ' Newton, efué realmente newtoniano en su filosofía científica ? ', *Theoria*, 1952–53, **1**, 164

James K. Feibleman, ' On the Theory of Induction ', *Philosophy and Phenomenological Research*, 1954, **14**, 332

Robert Feys, ' Nature et possibilités de la logique formalisée, *Theoria*, 1952–53, **1**, 133

Antonio Alvarez de Linera, ' La evolución y la unidad específica del hombre ', *Theoria*, 1953, **2**, 113

Miguel Sánchez Mazas, 'Un famoso símbolo matemático : la espiral de Bernoulli', *Theoria*, 1952-53, **1**, 128
Eugenio d'Ors, 'Los argumentos de Zenón de Elea y la noción moderna del espacio-tiempo', *Theoria*, 1953, **2**, 1
Emilio Palafox, 'Hacia una síntesis evolucionista', *Theoria*, 1952-53, **1**, 121
Carols Paris, 'Antropología y epistemología', *Theoria*, 1952-53, **1**, 137
Ramon Crespo Pereira, 'Algunas consideraciones sobre los signos y los símbolos', *Theoria*, 1953, **2**, 103
R. Rodriguez Vidal, 'Sobre el razibar en geometría', *Theoria*, 1953, **2**, 155
L. L. Whyte, 'A Hypothesis regarding the Brain Modifications Underlying Memory', *Brain*, 1954, **77**, 158
L. L. Whyte, 'The Two Philosophies : Atomism and Pattern', *Theoria*, 1953, **2**, 11 (also in Spanish p. 7)
L. L. Whyte, 'On the History of Natural Lengths', *Annals of Science*, 1954, **10**, 20
John W. Yolton, 'The Dualism of Mind', *The Journal of Philosophy*, 1954, **51**, 173
Juan Zaragüeta, 'La psico-física como preliminar a la física', *Theoria*, 1952-53, **1**, 147

Meetings of the Philosophy of Science Group

The following meetings were held during the academic year, 1953-4. They took place, by kind permission, in the Joint Staff Common Room, University College, London.

1953

12 October : Address by the Chairman (Mr L. L. Whyte), 'A Dimensionless Physics ?'
9 November : Dr N. Tinbergen, 'The Biological Study of Animal Behaviour : its possibilities and its limitations'
7 December : Professor J. B. S. Haldane, F.R.S., 'Communication between Animals'

1954

11 January : Professor Sir Harold Jeffreys, F.R.S., 'The Present Position in Probability Theory'
8 February : Professor F. E. Zeuner, 'The Domestication of Animals by Early Man : its psychological and social aspects'
1 March : Annual General Meeting
17 May : Professor Willard V. Quine, 'The Nature of Mathematical Truth'
14 June : Professor R. O. Kapp and Dr H. Bondi, 'Theories about the Origin and Disappearance of Matter'